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BIOMETRIC STUDIES OF THE GROWTH OF CHILDREN OF ROCHESTER, MINNESOTA

THE FIRST YEAR OF LIFE

BY MILDRED NORVAL, M. D.
First Assistant in Pediatrics,

ROGER L. J. KENNEDY, M. D.,
Section on Pediatrics,

AND

JOSEPH BERKSON, M. D., D. Sc.,
Division of Biometry and Medical Statistics,
Mayo Clinic,
Rochester, Minnesota

THIS report is the first of a series which will be submitted, presenting an analysis of measurements of length and weight, during the growth period, of children of Rochester, Minnesota. The data were obtained from the accumulated individual records in the schools of that city and its well-baby clinics, supplemented by birth and medical records in the history files of the Mayo Clinic. The present report refers to measurements from birth to the end of the first year of age.

The measurements were made during the period 1934 through 1945.

TABLE 1

Physical measurements of boys

AGE, (months)	NUMBER	LENGTH			WEIGHT			CORRELATION OF LENGTH AND WEIGHT
		Mean (inches)	S. D.* (inches)	C. V.† (per cent)	Mean (pounds)	S. D. (pounds)	C. V. (per cent)	
Birth	356	20.2	1.07	5.3	7.6	1.28	16.8	0.60
0- 1	541	20.7	1.01	4.9	8.5	1.26	14.8	0.80
1- 2	1,394	21.5	1.10	5.1	9.9	1.61	16.3	0.72
2- 3	1,432	22.9	1.15	5.0	12.1	1.72	14.2	0.71
3- 4	1,322	24.1	1.10	4.6	13.9	1.94	14.0	0.63
4- 5	1,179	25.2	1.01	4.0	15.4	1.78	11.6	0.65
5- 6	1,058	26.0	1.05	4.0	16.8	2.04	12.1	0.65
6- 7	1,041	26.7	1.02	3.8	18.0	2.09	11.6	0.62
7- 8	899	27.4	1.03	3.8	19.2	2.28	11.9	0.58
8- 9	851	27.9	1.05	3.8	20.2	2.30	11.4	0.62
9-10	773	28.4	1.09	3.8	20.9	2.40	11.5	0.61
10-11	686	28.8	1.10	3.8	21.7	2.48	11.4	0.63
11-12	581	29.4	1.19	4.0	22.4	2.48	11.1	0.59

* Standard deviation.
† Coefficient of variation.

TABLE 2

Physical measurements of girls

AGE, (months)	NUMBER	LENGTH			WEIGHT			CORRELATION OF LENGTH AND WEIGHT
		Mean (inches)	S. D. (inches)	C. V. (per cent)	Mean (pounds)	S. D. (pounds)	C. V. (per cent)	
Birth	335	19.9	1.01	5.1	7.3	1.26	17.3	0.55
0- 1	498	20.4	0.93	4.6	8.2	1.11	13.5	0.73
1- 2	1,201	21.2	0.96	4.5	9.4	1.26	13.4	0.68
2- 3	1,187	22.5	1.01	4.5	11.2	1.54	13.8	0.66
3- 4	1,095	23.6	1.01	4.3	12.8	1.67	13.0	0.63
4- 5	1,013	24.6	1.01	4.1	14.2	1.67	11.8	0.58
5- 6	927	25.4	0.99	3.9	15.6	1.81	11.6	0.60
6- 7	861	26.1	0.94	3.6	16.7	1.91	11.4	0.62
7- 8	803	26.6	1.02	3.8	17.6	2.09	11.9	0.62
8- 9	721	27.2	1.00	3.7	18.6	2.11	11.3	0.64
9-10	671	27.7	1.03	3.7	19.3	2.16	11.2	0.58
10-11	598	28.2	1.02	3.6	20.0	2.16	10.8	0.57
11-12	495	28.7	0.98	3.4	20.8	2.33	11.2	0.62

The children included in the study are predominantly of North European ancestry and have grown up in a rural farming community. They comprise a fair cross-section of that community, which is of average or somewhat above-average economic status.

Measurements were made of the children without clothes; weight in pounds was measured to 1/10 pound; length in inches was measured to 1/4 inch; age was expressed in the analysis as of last completed month of age. Only cases with simultaneous measurements of length and weight were included. For the present report, referring to the first year of life, there were 4,111 children, 2,079 boys and 2,032 girls. For the boys there were a total of 12,113 length-weight measurements, for the girls 10,405 such measurements. In Tables 1 and 2 are shown the numbers for each age, togegther with the means, standard deviations, coefficients of variation and correlations of length and weight.

MEAN VALUES

In Fig. 1 is shown the age trend for mean length and in Fig. 2, the age trend for mean weight. Curves have been drawn through the observed means graphically, and these indicate a slightly convex downward curvature in the period up to about two months and an upwardly convex regression from that month of age. These characteristics are reflected in the trend of the growth rates discussed later, in which a maximum occurs at 1 to 2 months.[1] The boys are consistently longer and heavier than the girls at each month of age. The difference in length increases from 0.3 inch at birth to 0.7 inch at the end of the first year. Expressed relatively as a percentage of the mean length, the excess of mean length of the boys is at birth about 1.4 per cent, rises to about 2.5 per cent by the fifth month, and remains at about that level to the end of the year. The comparison with respect to mean weight is similar in character. At birth the boys are about an average of 0.3 pound heavier than the girls;

[1] We fitted a third degree polynomial to the observations, as Lewis-Faning and Milligan, and Hill and Magee, did for their data. However, this function, while it provides a "good fit" to the observations by the criterion of statistically significant deviations, does not, for any of the data, depict the convex downward character of the curve in the first two months of life. This characteristic of the curve, reflecting *an increase of the growth rate* in this period, is confirmed by direct studies of the growth rate, such as those of Evetzky and the observations dealt with later in this paper in respect of our own observations. In the first week of life there is on the average a loss of weight; thereafter, the growth rate is positive, accelerating to a maximum at about the seventh week.

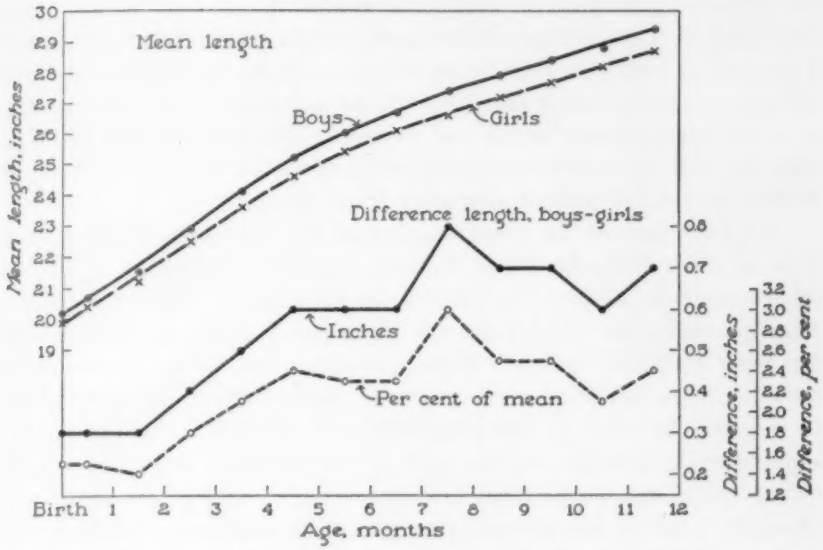

FIG. 1. AGE TREND OF MEAN LENGTH OF BOYS AND GIRLS

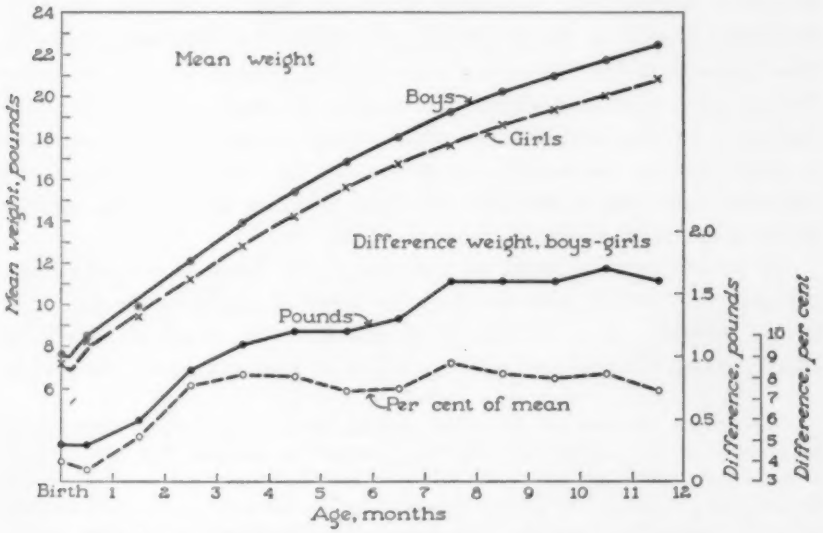

FIG. 2. AGE TREND OF MEAN WEIGHT OF BOYS AND GIRLS

the difference increases to about 1.6 pounds at the end of the year. Expressed as a percentage of the mean weight, the difference is about 4 per cent at birth, increases to about 7 per cent by the third month and remains at approximately that level till the end of the year. The finding of a uniformly greater length and weight in this pe .d of life for boys than for girls is in agreement with previous studies of other workers, as is also the general pattern of growth trend on age.

A comparison of the mean weights of the Rochester children with those of other series for white children, of the United States and some other countries, is shown in Table 3 for boys and in Table 4 for girls. Very generally, the weights for the Rochester children are higher than for other published series.[2] Some exceptions are found in the early months of life, when for some series the Rochester children are lighter in weight. But even in these the difference is made up for by a more rapid growth of weight on the part of the Rochester children, so that by the end of the year, except in only one series, that of a selected and therefore possibly unrepresentative group of California children, the mean weight for the Rochester group is larger than for other groups. We shall not venture any detailed explanation of the general superiority in weight of the group studied. It is to be presumed that the greater weight of the children reflects better nutrition and better environmental conditions during this important period of growth. This is probably attributed largely to the general improvement that has taken place in these respects in the last several decades, and perhaps also to the better than average economic status of the present group, to the fact that Rochester (with a population of about 30,000 persons) is predominantly a rural farming community, as well as to the excellent prenatal and postnatal care that is available to these children through the services of the large medical clinic situated in the city.

In preceding paragraphs we have traced the trend of weight on age, irrespective of length, and similarly the trend of length on age, irrespective of weight. It is of interest to delineate the trend of weight on length, irrespective of age, and the trend of age on length, irrespective

[2] The mean weights in our series are uniformly lower than those shown graphically in the "charts for pediatric practice" by Jackson and Kelly. However, in the publication presenting these charts no actual figures are given and the graphs presented are labeled "median." We have been informed through personal communication with the authors that the standards of these charts, since publication, have been somewhat modified in the direction of smaller values.

TABLE 3

Comparison of mean weight of children of the Rochester series with other series, boys

GROUP	AGE (months)											
	1-2	2-3	3-4	4-5	5-6	6-7	7-8	8-9	9-10	10-11	11-12	
Rochester, mean weight, pounds	9.9	12.1	13.9	15.4	16.8	18.0	19.2	20.2	20.9	21.7	22.4	
	Difference* from Rochester mean (pounds)											Mean
Woodbury, 1919, U. S. large series	+1.0	+0.5	+0.2	−0.2	−0.2	−0.5	−0.9	−1.2	−1.5	−1.6	−1.7	−0.6
Baldwin, 1921, Baltimore, Maryland, white children	−1.4	−1.4	−1.9	−2.1	−2.7	−2.5	−2.4	−2.9	−2.7	−2.4	−3.0	−2.3
Grover, 1915, Boston, Massachusetts, outpatients	−2.2	−3.2	−4.5	−5.9	−5.1	−1.7	−1.4	−3.6	−3.8	−5.0	−4.3	−3.7
Bayley and Davis, 1935, Berkeley, California, selected children	−0.3	−0.5	−0.6	−0.6	−0.3	−0.1	+0.1	+0.3	+1.4	+0.9	+1.0	+0.1
Peatman and Higgons, 1938, optimal care, suburban New York	−0.6	−0.7	−0.5	0	0	0	−0.2	−0.2	0	−0.4	−0.4	−0.3
Rude, 1922, Gary, Indiana, children	+0.9	+0.2	−0.4	−1.1	−0.9	−1.7	−1.9	−1.4	−1.9	−1.8	−2.6	−1.1
Schmid-Monnard, 1886-1889, normal German children	−2.3	−3.0	−3.2	−2.9	−3.8	−3.0	−3.7	−4.4	−4.2	−3.4	−3.8	−3.4
Camerer, 1893, German children	−1.0	−1.3	−1.3	−1.4	−1.7	−1.9	−2.1	−2.0	−1.8	−2.5	−2.1	−1.7
Höjer, 1920-1924, Swedish breast-fed children	−1.2	−1.4	−1.7	−1.4	−1.6	−1.3	−1.6	−2.0	−1.8	−2.0	−2.1	−1.6

Robertson, 1911-1914, English welfare children	−0.7	−1.5	−1.8	−1.7	−2.2	−2.3	−2.4	−2.9	−3.2	−3.0	−3.4	−2.3
Hill and Magee, 1933-1936, English welfare children	−1.5	−2.0	−2.0	−2.2	−1.8	−1.9	−2.1	−1.8	−1.4	−1.5	−1.7	−1.8
Gore and Palmer, 1949, London welfare children	−1.1	−1.0	−0.8	−0.8	−0.7	−0.4	−0.6	−0.5	−0.5	−0.6	0	−0.6
Lewis-Faning and Milligan, 1940-1942, Glossop welfare children	−1.5	−2.0	−1.8	−1.7	−1.4	−1.5	−1.6	−1.7	−1.1	0	+0.7	−1.2
Karn and Pearson, 1922, English welfare children	−1.4	−2.2	−2.7	−3.1	−3.6	−3.6	−4.0	−4.3	−4.4	−4.5	−4.4	−3.5
Paton and Findlay, 1926, poor in Scottish cities	−1.3	−2.4	−2.6	−2.7	−2.8	−3.1	−3.5	−3.5	−3.6	−3.8	−4.0	−3.0
Guerra, 1950, high economic status, Uruguay	−0.9	−1.1	−1.1	−1.0	−1.0	−1.0	−1.1	−1.0	−0.8	−0.7	−0.6	−0.9

* A − sign signifies that the mean weight for the series referred to was less than that for the Rochester series, a + sign that it was greater.

TABLE 4

Comparison of mean weight of children of the Rochester series with other series, girls

GROUP	AGE (months) 1-2	2-3	3-4	4-5	5-6	6-7	7-8	8-9	9-10	10-11	11-12	
Rochester, mean weight, pounds	9.4	11.2	12.8	14.2	15.6	16.7	17.6	18.6	19.3	20.0	20.8	
	Difference* from Rochester mean, pounds											Mean
Woodbury, 1919, U. S. large series	−0.9	−1.1	−1.1	−1.2	−1.3	−1.3	−1.4	−1.5	−1.5	−1.7	−1.9	−1.4
Baldwin, 1921, Baltimore, Maryland, white children	−0.4	−1.4	−2.0	−2.1	−3.4	−2.3	−2.1	−2.8	−2.4	−2.3	−1.8	−2.1
Grover, 1915, Boston, Massachusetts, outpatients	−2.4	−3.6	−3.2	−3.2	−0.7	−0.6	−2.6	−1.5	−2.6	0	−1.8	−2.0
Bayley and Davis, 1935, Berkeley, California, selected children	−0.6	−0.6	−0.7	−0.7	−0.4	−0.5	0	−0.1	0	+0.2	+0.2	−0.3
Peatman and Higgons, 1938, optimal care, suburban New York	−0.5	−0.4	−0.3	+0.3	+0.1	0	0	+0.2	+0.4	+0.6	+0.2	0
Rude, 1922, Gary, Indiana, children	+0.1	+0.1	−0.2	−0.9	−1.5	−1.9	−1.4	−1.9	−1.9	−2.9	−2.6	−1.4
Schmid-Monnard, 1886-1889, normal German children	−2.3	−2.4	−2.2	−2.3	−2.7	−2.5	−2.5	−3.3	−3.0	−3.4	−4.0	−2.8
Camerer, 1893, German children	−1.0	−1.3	−1.5	−1.5	−1.3	−1.6	−1.4	−1.5	−1.6	−1.5	−1.4	−1.4
Höjer, 1920-1924, Swedish breast-fed children	−0.7	−0.8	−0.8	−1.1	−1.3	−1.5	−1.0	−1.2	−1.1	−1.0	−1.0	−1.0

Robertson, 1911-1914, English welfare children	−0.5	−1.2	−1.6	−1.6	−2.0	−2.0	−1.8	−2.4	−2.7	−2.9	−2.8	−2.0
Hill and Magee, 1933-1936, English welfare children	−1.4	−1.7	−1.4	−1.5	−1.6	−1.6	−1.4	−1.1	−1.0	−1.0	−1.2	−1.4
Gore and Palmer, 1949, London welfare children	−0.7	−0.8	−0.7	−0.5	−0.3	−0.2	−0.3	−0.1	−0.2	+0.2	−0.1	−0.3
Lewis-Faning and Milligan, 1940-1942, Glossop welfare children	−1.4	−1.6	−1.9	−1.6	−1.2	−1.5	−1.5	−0.6	−0.8	+0.2	−0.1	−1.1
Karn and Pearson, 1922, English welfare children	−1.3	−1.7	−2.5	−2.7	−3.0	−3.2	−3.3	−3.7	−3.9	−4.1	−4.3	−3.1
Paton and Findlay, 1926, poor in Scottish cities	−1.3	−2.1	−2.5	−2.5	−2.8	−2.9	−2.9	−3.4	−3.1	−3.2	−3.6	−2.8
Guerra, 1950, high economic status, Uruguay	−0.8	−1.0	−1.1	−1.0	−1.1	−0.9	−0.7	−0.6	−0.4	−0.2	−0.3	−0.7

* A − sign signifies that the mean weight for the series referred to was less than that for the Rochester series, a + sign that it was greater.

of weight. The first would answer a question referring to the prediction of the weight, given length alone, and the second a question referring to the prediction of age, given length alone.

In Fig. 3 is shown graphically the mean weight for length separately

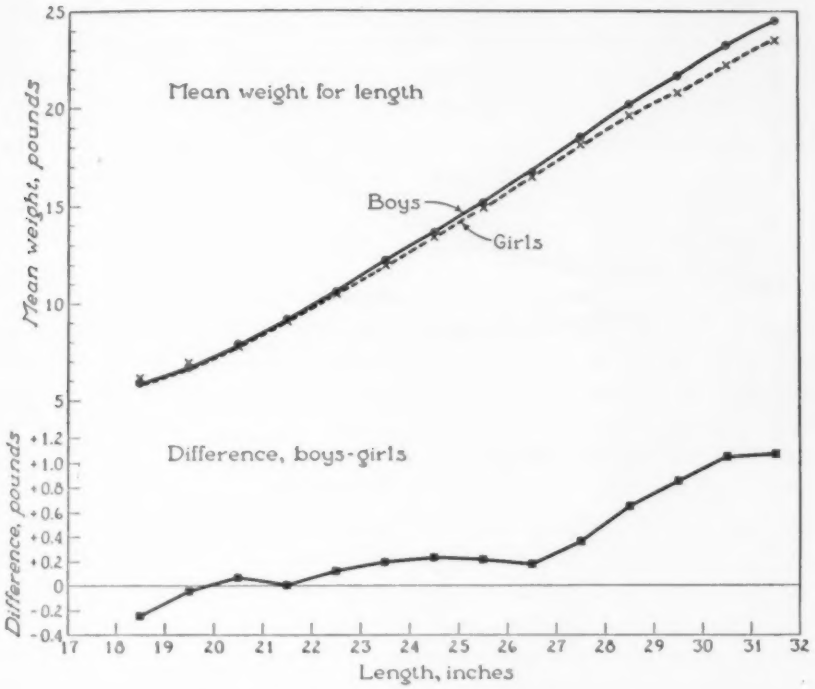

FIG. 3. MEAN WEIGHT FOR GIVEN LENGTH

for boys and girls and the difference between them. It was observed previously that for given age the boys are heavier than the girls and that also the mean length is greater for boys than for girls. In the present figure we see that even for the same length the boys are in general heavier than the girls. In Fig. 4 is shown the mean age for length. For any specified length the boys are younger than the girls.

Taking the two figures together we see that for any length the boys are on the average heavier but younger than the girls. The numerical values corresponding to Figs. 3 and 4 are shown in Table 5. The finding that, for given length, boys are heavier than girls confirms the observations reported by Woodbury, by Kornfeld and by Baldwin. The difference

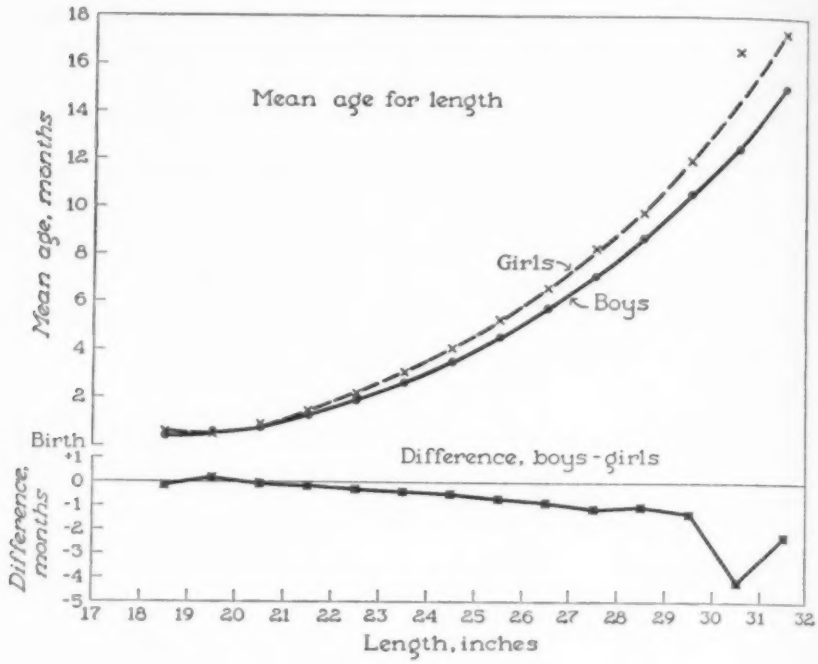

FIG. 4. MEAN AGE FOR GIVEN LENGTH

TABLE 5

Mean weight and age, for given length

LENGTH (inches)	WEIGHT (pounds)		AGE (months)	
	Boys	Girls	Boys	Girls
18-19	5.9	6.2	0.4	0.5
19-20	6.9	6.9	0.6	0.5
20-21	7.9	7.8	0.8	0.9
21-22	9.2	9.2	1.3	1.4
22-23	10.6	10.5	1.9	2.2
23-24	12.2	12.0	2.7	3.1
24-25	13.6	13.4	3.5	4.0
25-26	15.2	14.9	4.5	5.2
26-27	16.8	16.6	5.7	6.6
27-28	18.5	18.1	7.1	8.3
28-29	20.2	19.6	8.7	9.7
29-30	21.7	20.9	10.5	11.9
30-31	23.3	22.3	12.5	16.6
31-32	24.6	23.6	15.1	17.4

is very small, hardly apparent at all until a length of 22 inches is attained, when it amounts to a mean of 0.1 pound, and increases to about one pound at the attained length of 30 inches. The finding of a younger mean age for length of the boys as compared with the girls corresponds, of course, to the finding previously referred to, of a greater length for age, of the boys compared with the girls. The difference is in fact hardly perceptible till a length of 22 inches is reached, when it amounts to only 0.3 month, and at 29 to 30 inches it amounts to about 1.5 months. Thompson found in her study of 101 infants between the ages of 8 and 56 weeks that the girls were about one month older than the boys at the time of attaining the same length.

VARIABILITY

The variability measured as standard deviation (Tables 1 and 2) in respect to length shows no obvious trend during the year and is somewhat larger for boys (mean standard deviation 1.08 inches) than for girls (mean standard deviation 0.99 inch). For weight the standard deviation increases somewhat during the year for both boys and girls and is on the average larger for boys (mean standard deviation 2.0 pounds) than for girls (mean standard deviation 1.8 pounds). For length Woodbury found some increase in standard deviation with age as did also Vickers and Stuart, Meredith, and Hill and Magee, and no difference between the sexes, while Simmons found a larger variability for girls than for boys, and these are in contrast with our findings. As regards weight the findings of these authors as well as Robertson, Peatman and Higgons, Gore and Palmer, and Simmons are in agreement with ours in their directional trends.

The variability measured relatively by expressing the standard deviation as a percentage of the mean, to give the coefficient of variation, shows a decrease during the first year of life for both boys and girls in respect to both length and weight. This is the result, arithmetically, of the fact that the absolute variability increases during this period at not so rapid a rate as does the mean length and weight. Biologic implications of the decrease of the coefficient of variation are discussed in the last section of this paper. The mean relative variability is somewhat larger for boys than for girls in respect to length (boys 4.3 per cent and girls 4.0 per cent) and to weight (boys 13.0 per cent, girls 12.5 per cent).

The variability shown in this series is smaller than that found

generally in other studies reporting large series of data. Thus the mean standard deviation for length in the first year for this Rochester group is about 1.0 inch, while for the series of Vickers and Stuart it is 1.7 inches, for Woodbury's series it is 1.4 inches, and for Hill and Magee's series it is 1.1 inches. For weight, the mean standard deviation is about 1.8 pounds for the Rochester group, for Woodbury's series it is about 2.3 pounds, for Hill and Magee's series it is 1.9 pounds and for the series of Vickers and Stuart about 2.1 pounds. The smaller variabilities

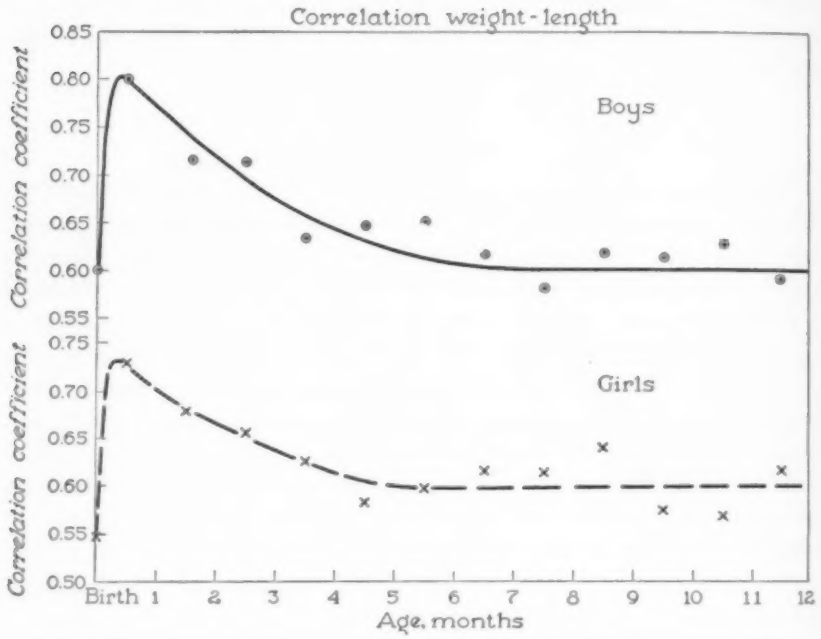

FIG. 5. CORRELATIONS BETWEEN WEIGHT AND LENGTH AT DIFFERENT AGES

generally characterizing our findings as compared with those reported by other workers are probably a reflection of the greater homogeneity of the Rochester series than of the other series, in that it consists largely of measurements of the same children, and also of the general uniformity of racial composition of the group, and of the environmental conditions obtaining during the growth period.

CORRELATION

The product-moment correlation coefficient for length and weight was calculated from the available data for each month of age, separately for

the boys and for the girls. The values are shown in Tables 1 and 2, and graphically in Fig. 5. At birth the correlation is for boys 0.60, for girls 0.55. Bakwin and Bakwin found the correlation between length and weight at birth to be 0.66 for boys and 0.72 for girls. Pearson found the correlation for boys to be 0.64 and for girls to be 0.62, Bayley and Davis found the correlation to be 0.78 for boys and 0.81 for girls, Taylor found the correlation to be 0.79 for boys and 0.81 for girls. We see, therefore, that there is considerable disagreement respecting the general level of the correlation, and also with regard to the relative magnitude for boys and girls.

In the present data the correlation rises rapidly in the first month. About 0.6 at birth, it is at the end of the month much higher, 0.80 for boys, 0.73 for girls. The lower correlation found at birth than later is related probably to the difficulty of measuring length at birth with standardized precision. The infant is held by the feet against a tape and the position of the occiput is taken to give length. For the measurement at a month's age, the infant is placed on its back on an enclosed measuring board, and these measurements are probably better standardized than those at birth. From the first month of age there is a gradual decrease of the correlation to a level of about 0.6 attained at about the sixth month of age, and thereafter it remains at about this level for the rest of the year. This trend is similar for the boys and girls, but the correlation for the boys is slightly larger than for the girls, the mean difference being 0.03 ± 0.01. Simmons found the trend of correlation on age to be similar for boys and girls, the correlation for the boys being higher than that for the girls. Berkson found, from calculations of Woodbury's data for males, a decrease of the correlation till about the fifth month of age, after which there was a rise. A similar trend of a decrease followed by a rise in the last half of the year was reported by Bayley and Davis, but the present study does not exhibit this phenomenon.

RATE OF GROWTH

The mean monthly increments of growth in weight can be obtained by successive subtraction of the monthly mean values given in Tables 1 and 2. However, this method will not reveal any information regarding the *variability* of the growth rates among individuals. To obtain this it is necessary to calculate the increments for each individual separately. Because of the importance of the datum of variability, we obtained, for

each individual, each monthly increment of weight, and what is here presented respecting the rate of growth of weight is based on these data. The individuals included are the same as those utilized for the calculations referred to previously except for weight at birth. In what has been so far dealt with, for each month an individual was included only if there was available a measurement of both weight and length. However, there was a considerable number of cases in our series in which birth weight, but not length, was recorded. These cases were included for the study of rate of growth in weight during the first month of age. Since each difference required the recording of the weight at two successive months, the omission in the record of the weight for a particular month results in the omission of two increments. For this reason the number of observations used to obtain the differences for the monthly intervals is considerably smaller than the numbers used to obtain the means themselves, and the mean differences are slightly at variance with the differences of means.

The mean growth rates of weight per month and the variability of these are presented in Table 6. In Fig. 6 is shown graphically the trend on age of the mean growth rate of weight per month, expressed in absolute and relative terms. The rate is slightly greater for the boys than for the girls on either basis. The difference decreases with age and the rates are about equal by the end of the year's growth. The general trend is fairly clear. From birth, the growth rate rises somewhat to a maximum of 2 to 2.5 pounds per month at age 1 to 2 months. From this point it decreases rather rapidly in the next two or three months to little more than half this amount and thereafter decreases more gradually.

The variability of the rates of growth among different children, measured as standard deviation, is, during the first month, about 1 pound, somewhat larger for boys than girls. Thereafter it decreases steadily for both boys and girls, reaching a level of about 0.6 pound by the end of the year (Table 6). Measured as coefficient of variation, the variability of the growth rates remains fairly constant at a level of about 55 per cent.

Although, as stressed early by Evetzky, the growth rate is a very revealing index of the growing child's well-being, the hazard of using the mean values of the growth rate as an absolute standard, to which particular individuals must adhere, is apparent in the size of the variability among the different normal individuals who go into the establishment of these mean rates. The mean growth per month in the

TABLE 6

Monthly rate of growth of weight

INTERVAL OF GROWTH (months)	BOYS				GIRLS			
	Number	Mean (pounds per month)	S. D. (pounds per month)	Growth rate (per cent)	Number	Mean (pounds per month)	S. D. (pounds per month)	Growth rate (per cent)
Birth- 0.75	507	1.3	0.83	17.1	353	1.1	0.69	15.1
0.75- 1.5	388	2.4	1.17	28.2	260	2.0	0.93	24.4
1.5 - 2.5	1,015	2.4	0.97	24.2	882	2.1	0.91	22.3
2.5 - 3.5	953	1.9	0.79	15.7	840	1.8	0.76	16.1
3.5 - 4.5	850	1.7	0.79	12.2	591	1.6	0.77	12.5
4.5 - 5.5	714	1.5	0.74	9.7	565	1.5	0.72	10.6
5.5 - 6.5	619	1.3	0.66	7.7	485	1.2	0.59	7.7
6.5 - 7.5	571	1.2	0.70	6.7	462	1.1	0.55	6.6
7.5 - 8.5	481	1.0	0.55	5.2	421	1.1	0.65	6.3
8.5 - 9.5	454	1.0	0.58	5.0	287	0.9	0.53	4.8
9.5 -10.5	358	0.8	0.59	3.8	309	0.9	0.89	4.7
10.5 -11.5	297	0.9	0.61	4.1	244	0.9	0.65	4.5

first year of life is about 1.5 pounds, while the standard deviation measuring the variability is about 0.7 pound or almost 50 per cent of the mean. This means that about a quarter of the children gain less than about 1 pound per month, and a quarter gain more than 2 pounds per month. Unless the gain of a child has been considerably beyond these limits, therefore, it is hardly a reason in itself for regarding the growth as abnormally deficient or excessive.

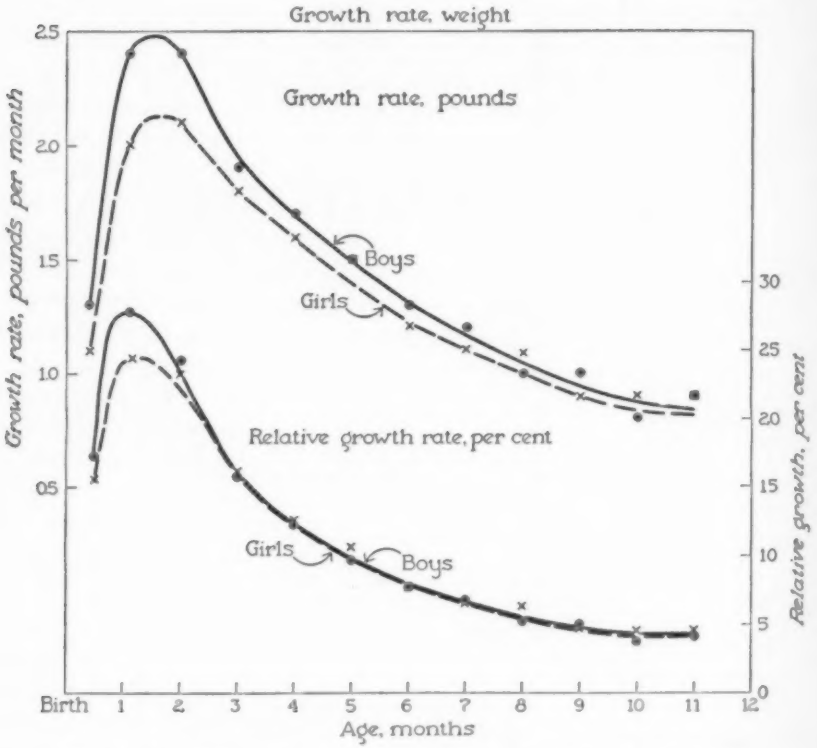

FIG. 6. AGE TREND OF MEAN GROWTH RATE OF WEIGHT PER MONTH

Our finding of a greater growth rate for boys than for girls is in agreement with observations of Thompson, Beskow, Faber, Woodbury. Hill and Magee, Höjer, Schiötz and Berghoff, Lewis-Faning and Milligan, and Simmons. Bayley and Davis found no sex difference. Studies which have examined particularly the first several weeks of life show the acceleration of growth (after an initial loss of weight in the first week) that

we found. Following the first two months practically all studies show a continuous deceleration of the growth rate.

As an item of evidence of what is accomplished by improved nutritional status in affecting growth in the first year of life, we may set a few statistical figures obtained in this study against some given by Evetzky in 1881 in a study of growth rates referring to children from the New York Infant Asylum considered quite normal. The average birth weight for the boys in the New York group was 7.3 pounds, as contrasted with 7.6 pounds for the present study, and for the girls 7.0 pounds contrasted with 7.3 pounds. Handicapped therefore as respects weight at birth, the New York children were further retarded with respect to their growth rates. In the first month of life, both series showed an acceleration of growth rate, the smoothed mean figure for this series being 1.4 pounds per month, the corresponding figure for the New York group being 0.6 pound per month (2.56 ounces per week). Similarly the comparative figures for the growth rate in the successive following months show consistently an average rate of about 0.4 pound per month less for the New York group than for the present series. By the end of the fourth week, in the New York series 20 per cent of the children showed a weight less than that at birth, these not having overcome the characteristic loss occurring immediately after birth; none exhibited such a deficiency in the present series.

The monthly growth in length was estimated from the differences of the mean lengths attained at the various ages in contrast with the estimates of growth rate of weight, for which the differences of the individual children were used. The growth rates of length thus obtained are shown in Fig. 7 with smooth curves drawn through the observed values by eye. The trends in general have the same character as the corresponding curves for growth rate of weight. The rates are greater for boys than for girls, the difference diminishing steadily with age. On a relative basis, measured percentagewise, the growth rate of length is of course much smaller than that of weight, the dimensions of weight being the cube of length.

RELATION OF MEASURES AT BIRTH TO LATER MEASURES

It is an old observation that the weight of an infant at the end of the first year's growth depends on the weight at birth, those children who are light at birth generally being light at the end of the year, and

vice versa for the infants who are heavy at birth. For instance, a very clear graphic presentation of this generality was given in 1893 by Camerer, who presented the curve of mean weight for months of age, separately for three groups of children, weighing at birth respectively less than 2 kg., 2 to 3 kg. and 3 to 4 kg. The curves are consistently one below the other, showing that the mean weights remain different throughout the year, and the differences between them are just as large

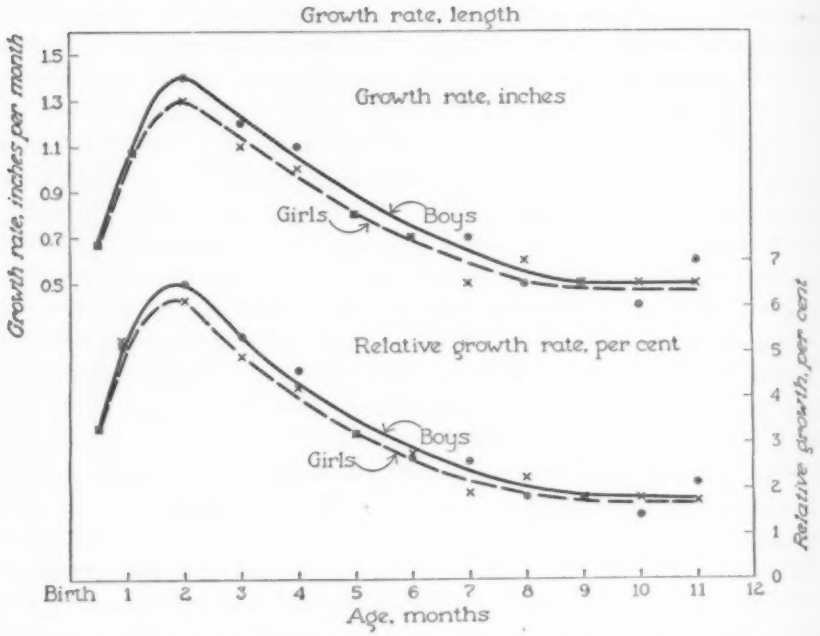

FIG. 7. AGE TREND OF MEAN GROWTH RATE OF LENGTH PER MONTH

or larger, at the end of the year, as at birth. This phenomenon has been studied by others and particular consideration has been given to it in some recent studies by Illingworth. Graphs similar to those of Camerer for both weight and length and separately for boys and girls, applying to the data of the present study, are shown in Figs. 8 and 9, where the mean values of weight and length at successive months of age are depicted for six groups differentiated in respect to birth weight. The curves maintain the relative position they had at birth throughout the year, except for occasional approach of the mean values, which may be

attributed to random fluctuations. That is, the group with the smallest weight at birth has the smallest mean weight through the year, the group with the next greater weight at birth has the next larger mean weight, and so forth, and similarly with respect to mean length. A comparison of the mean weights attained at the end of the year, for groups differentiated according to birth weight, is depicted in greater detail in Fig. 10,

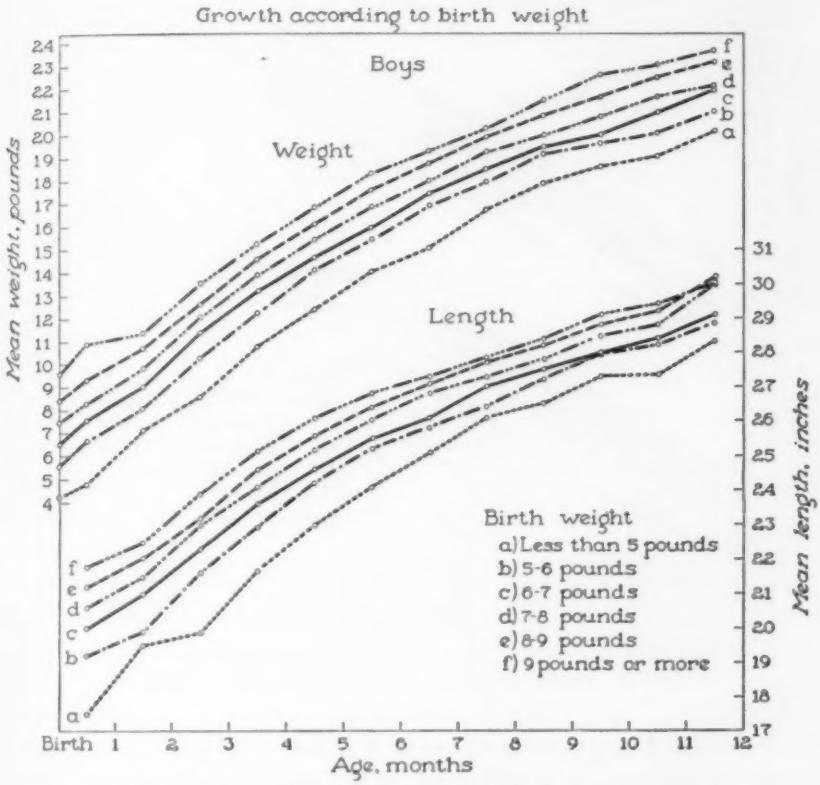

FIG. 8. GROWTH OF BOYS ACCORDING TO BIRTH WEIGHT

where the mean weight at 1 year is plotted against birth weights in increments of 1 pound units. Lines fitted to these points by least squares gave the regressions shown in the figure. The correlation coefficients are positive, indicating again the tendency for the individuals who are larger at birth to be larger at 1 year. However, the value of the correlation

coefficient is not high, for boys + 0.44 and for girls + 0.42, which carries the implication that the reduction of variability in weight at 1 year of all the children, if the children of fixed birth weight are considered, is only about 10 per cent. This is to say, that while the *mean* weights at

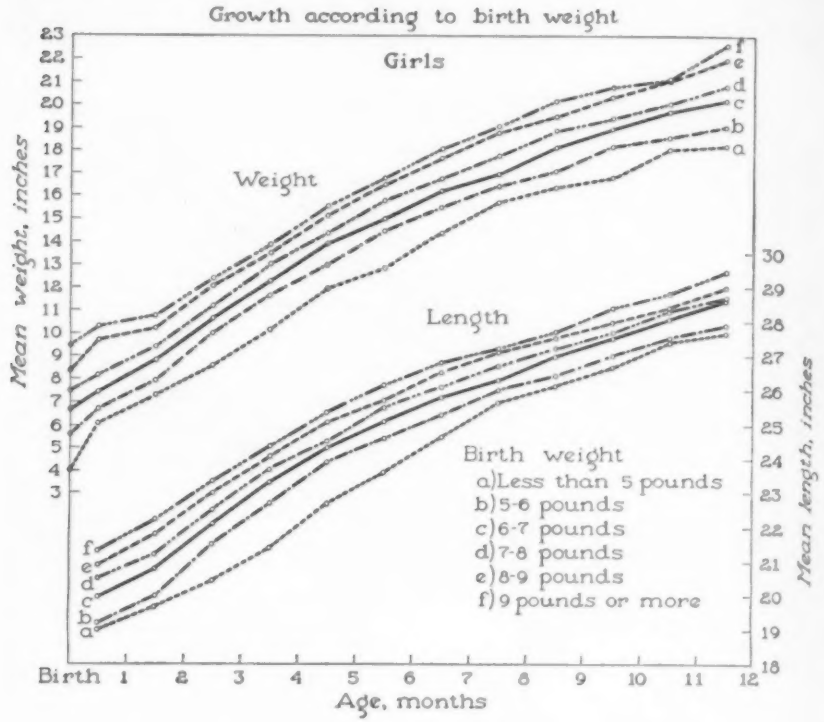

FIG. 9. GROWTH OF GIRLS ACCORDING TO BIRTH WEIGHT

1 year are different for the children of different birth weights, there is a great overlap of the distributions of weight, so far as the individuals are concerned. For instance, for a bivariate normal distribution, these correlations imply quantitatively that, of the children below average at birth, only 68 per cent will be below average at 1 year (Pearson); the corresponding observed figures were, for boys 67 per cent, and for girls 71 per cent.

Similarly we related the mean length at 1 year to the length at 1 to 2 months (birth lengths being available for only relatively few cases)

and these are shown in Fig. 11. The findings are similar to those observed for weight; the regressions are positive, the correlation coefficient being for boys + 0.49, for girls + 0.39.

We must not assume that the correlation between size at birth and that at later ages is necessarily the reflection of a carrying forward of nutritional or other environmental disadvantages present at birth. Part,

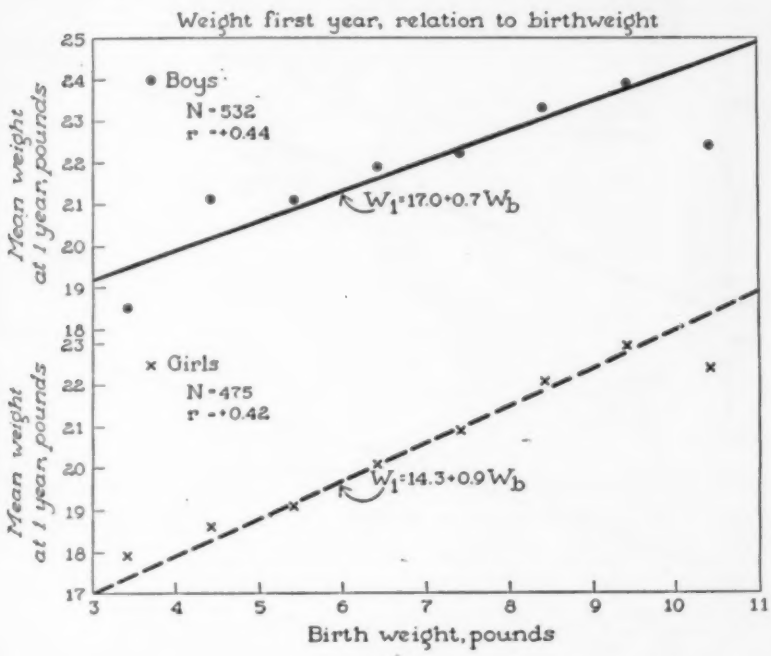

FIG. 10. REGRESSIONS OF WEIGHT AT 1 YEAR ON BIRTH WEIGHT

and possibly a large part, must reflect constitutional determinants of size. A child born of small parents will generally be small at birth and remain relatively small throughout life in contrast to a child of large parents, who will generally be large. To differentiate between the constitutional and the environmental elements that are the basis of the observed correlation would require observations relating to size of parent as well as to size of child.

As has been mentioned, the maintenance at 1 year of the relative position held at birth, shown in the means, is not exhibited by all the children, nor do those children who retain to 1 year a handicap or

advantage with respect to the average, held at birth, do so to the same degree. Some evidence of this may be obtained from an examination of the value of the coefficient of variation at 1 year as compared with its value at birth. If, for instance, each child gained the same percentage of its birth weight during the year, then the coefficient of varia-

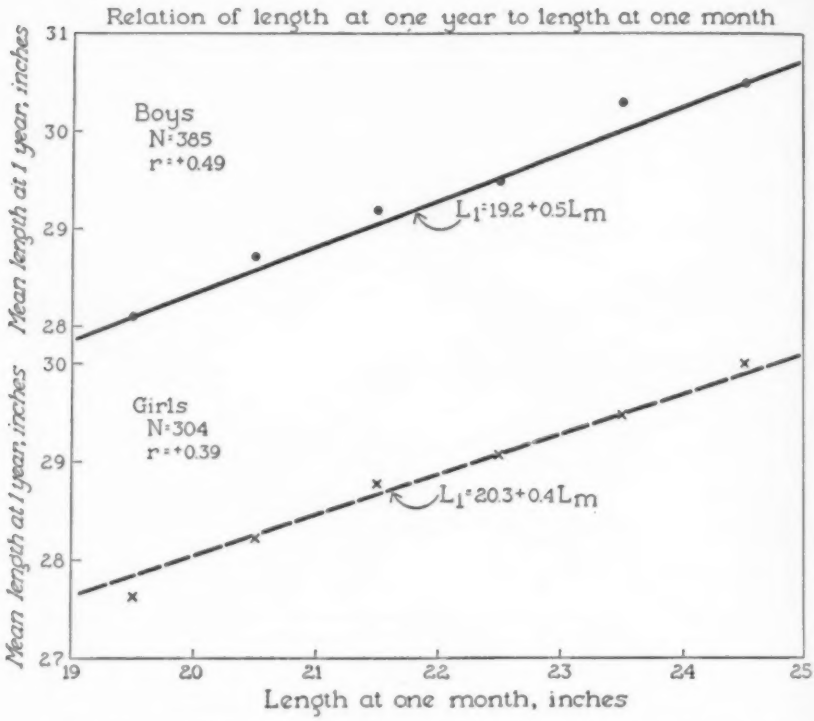

FIG. 11. REGRESSIONS OF LENGTH AT 1 YEAR ON LENGTH AT 1 MONTH

tion would be equal at the beginning and the end of the year. If, however, there was a tendency for the smaller children to gain a larger percentage of their weight than the heavier children, then a diminution of the coefficient of variation would result.[3] For the present series the comparisons of the coefficients of variation are as in Table 7.

[3] Essentially the same results are obtained if, instead of the coefficient of variation, one deals with the standard deviation of the measures in terms of their natural logarithm.

There is a reduction of the coefficient of variation by the end of the year for both weight and length. This is corroborated in the findings of other workers, such as Woodbury, and Hill and Magee. We may conclude from this that there is a general tendency to differentiation in respect to increase in weight or length, the individuals small at birth increasing by a larger amount *percentagewise* than the individuals who are large at birth. It has been suggested that the reduction of variability reflected in the reduction of the coefficient of variation, (or what amounts

TABLE 7

Coefficient of variation (per cent)

		BIRTH	1 YEAR	RATIO
Weight	Boys	16.8	11.1	0.66
	Girls	17.3	11.2	0.65
Length	Boys	5.3	4.0	0.75
	Girls	5.1	3.4	0.67

to the same thing, the absolute variability of the logarithms of the values themselves) indicates that there may be a compensating biologic mechanism at work, so that the smaller child is favored as against the larger. The reduction in the coefficient of variation in itself, however, is perhaps to be expected from arithmetic considerations, since even the same absolute increase in weight, for a small child as compared with a larger child, will appear as a larger *percentage* increase. That it is doubtful that this finding by itself can be considered as evidence of the existence of any biologic compensatory mechanism in the growth process has been demonstrated by Berkson and Schultz. More direct information on the question is obtained by correlating the birth weight with the absolute growth of weight during the year. The mean gain of weight in pounds plotted against the birth weight is shown in Fig. 12. For both boys and girls the regression is negative, corroborating the idea that small children have a larger increment of growth than the larger ones. However, within this general tendency, the variability among the individual children is very large, so that the correlation coefficient for birth

weight and gain in weight in the first year is small, — 0.15 for the boys and — 0.05 for the girls, the latter value not being statistically significant (at level P = 0.05).

SUMMARY

A study is presented of the biometric interrelationships and growth of length and weight during the first year of life. The data consist of 22,518 pairs of measurements of length and weight made on 4,111

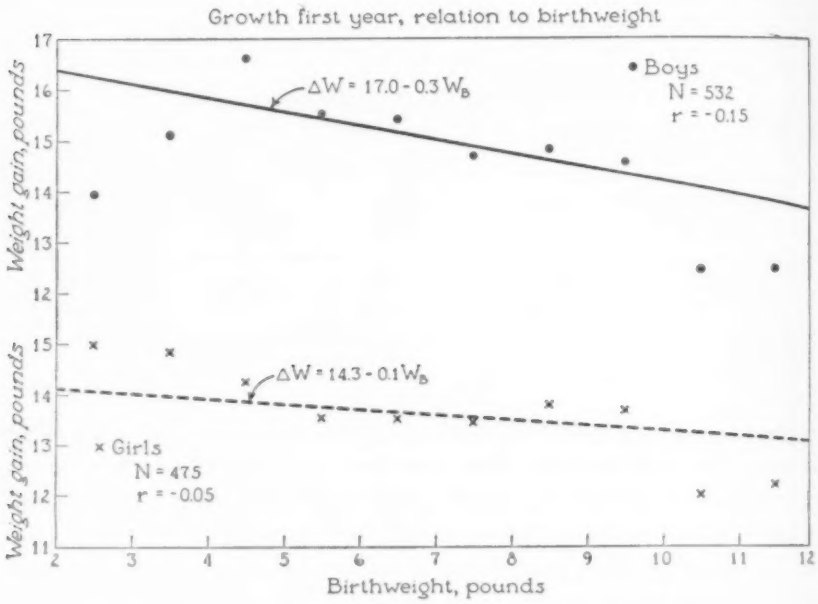

FIG. 12. REGRESSIONS OF GROWTH IN WEIGHT DURING FIRST YEAR ON BIRTH WEIGHT

children of Rochester, Minnesota. Boys are longer and heavier than girls of the same age, and boys are heavier than girls of the same length. The mean weights for the Rochester children are higher generally than for other previously published series. The variability of both length and weight, measured as standard deviation, or relatively as the coefficient of variation, is somewhat larger for boys than for girls; the relative variability decreases during the first year of life. The correlation of length with weight increases during the first month and then decreases gradually till about the sixth month, remaining at about the level then

attained till the end of the year. The growth rates of length and weight are somewhat larger for boys than for girls in the beginning, but approach equality by the end of the year. The variability of the growth rates, like the variability of the length and weight, decreases during the year. There is, in general, a negative relation between weight at birth and the mean growth during the year, that is, the amount of growth is larger for the smaller children than for the larger children. However, the variability among individual children in this respect is large, so that despite this tendency, children maintain to a considerable degree, to the end of the year, their relative position in respect to weight at birth; that is, in general, the larger children at birth are larger at 1 year. Evidence of a compensating biologic mechanism of growth tending toward greater uniformity of the group is discussed.

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GENERAL SYSTEM THEORY: A NEW APPROACH TO UNITY OF SCIENCE

1. PROBLEMS OF GENERAL SYSTEM THEORY[1]

BY LUDWIG VON BERTALANFFY

Department of Biology, University of Ottawa, Ottawa

IN HIS novel *Time Must Have a Stop*, Mr. Aldous Huxley introduces a gentleman whose concern is bridge-building between the dissevered parts of our spiritual life: physics and sociology, science and mysticism, power politics and world peace. The author, who feels like no one else the secret pulse of our epoch, makes us understand that this time of crisis needs new syntheses and a universal and profound reorientation—although in his private life Mr. de Vries is an uncurable fool. In this discussion let me also do some bridge-building, hoping, however, not to share all the shortcomings of Mr. de Vries.

Surveying the evolution of modern science, we find the remarkable phenomenon that similar general problems and viewpoints appear in the different branches of science. From its beginning, scientific thinking was governed by principles of an elementalistic type. This is natural because they represent the simplest modes of thinking. However, the progress in all sciences presents problems of a non-elementalistic or synholistic type. Thus, principles of wholeness, of organization, of dynamic interaction, appear in modern, as opposed to classical, physics; they appear in modern biology in the form of organismic conceptions in contrast to the analytical, summative, and machine-theoretical conceptions of the biology of yesterday; they are manifest in psychological *gestalt* theory, in contrast to classical association psychology, as well as in the modern conceptions of sociology. Thus, similar general viewpoints are present in all branches

[1] This and succeeding papers (Nos. 1-5) were presented at the Symposium held at the 47th Annual Meeting of the American Philosophical Association, Eastern Division, Toronto, Dec. 29, 1950. Paper No. 6 was not presented at the Symposium but is included here for evaluating different approaches to interscientific synthesis.

of science, irrespective of whether inanimate things, organisms, mental or social phenomena are concerned.

This parallelism is the more striking because these developments are mutually independent and largely unaware of each other, and differ in one important respect from the mechanistic conception of the nineteenth century. This parallelism is not based upon the Laplacean belief in a final reduction of all phenomena to physics and chemistry, and ultimately to a play of atoms, but rather we are aware of the autonomy of laws in the different levels of reality. The problem is of a catholic nature and it asks for a universal doctrine of "wholeness" and "organization."

Modern science is characterized by its highly developed specialization which became necessary because of the vast amount of facts and the complication of scientific techniques, experimental as well as theoretical. On the other hand, there is a strong tendency towards generalized systems of scientific laws which is based upon the insight that important developments frequently arise on borderlines and by way of a synthesis of formerly unconnected fields.

We may recall Koehler's (15) physical *gestalten*, taking into account physical systems, biological phenomena and the neurological basis of mental phenomena, supposedly based upon physical *gestalten*. Lotka (18) was probably the first to apply a concept of system which is general and not, like Koehler's, limited to systems in physics, though with him, as a statistician, we find the somewhat paradoxical situation that populations are conceived as systems, individual organisms, however, as "sums of cells." More recently, Cybernetics (24) was developed as a general theory, intended to embrace communication engineering, neurophysiology, psychology, and sociological aspects. Trimmer (22) has given a presentation of the "Response of Physical Systems." The notion of "system" being defined as "any arrangement or combination, as of parts or elements, in a whole," applies to a cell, a human being, a society, as well as to an atom, a planet, or a galaxy. Trimmer's treatment, closely linked with Cybernetics, is restricted to physical systems, which are the simplest for study, but he hints explicitly to a wider scope, emphasizing that the study of physical systems may pave the way towards that of non-physical ones. Lienau (17), starting from sociology, aims at a general quantitative theory of organization. His scope is identical with ours, although his mathematical approach (group theoretical considerations) is different from ours. Forms of a generalized kinetics have been used by von Bertalanffy (7, 8, 9) in the theory of metabolism and

growth, by Spiegelman (21) in a theory of competition and regulation in the organism, by Volterra and others (11, 16, 23) in the theory of biocoenoses and in demography. The concept of equilibrium is applied not only to physical, but also to biological and economic phenomena (12). Other examples of the same trend can easily be found in recent literature.

Thus there is a strong, though never clearly expressed, feeling that a generalization of scientific theory is necessary. The system of "laws of nature" was as yet almost identical with physics. It seems necessary to expand our conceptual schemes in order to establish systems of exact laws also in those fields where an application of physico-chemical laws is unfeasible.

These considerations lead us to postulate a new basic scientific discipline which we call General System Theory (3, 4, 5). A survey of some principles of General System Theory has been given recently (6). There are principles which apply to the entities called "systems" in general, whatever the nature of their component elements and the relations or forces between them. The fact that all sciences mentioned are concerned with "systems," leads to a formal correspondence or isomorphy in their general principles, and even in their special laws if the conditions correspond in the phenomena under consideration.

General System Theory would be an exact doctrine of wholeness as a "pure natural science" or *reine Naturwissenschaft*, to use Kant's expression,—that is, it is a hypothetico-deductive system of those principles which follow from the definition of system and by the introduction of more or less special conditions. In this sense, system theory is *a priori* and independent of its interpretation in terms of empirical phenomena, but is applicable to all empirical realms concerned with systems. Its position is similar to that, for example, of probability theory, which is in itself a formal mathematical doctrine but which can be applied, by way of empirical interpretation of its terms, to different fields, from games to thermodynamics, to biological and medical experimentation, to genetics, to life insurance statistics, and so on. Speaking more precisely, fields of application of system theory are all levels of science: first, the level of physical; second, of biological; third, of sociological units. Essentially it represents a generalized kinetics and dynamics which is applicable not only to physical systems, but to phenomena of any kind.

General System Theory allows logico-mathematical definitions of many ill-defined and much disputed concepts such as those of wholeness, summativity, emergent and resultant evolution, progressive segregation, mechanization and centralization, individuality, hierarchical order, con-

trolling parts, trigger action, competition, finality and equifinality, physical and biological time, and so forth. It can be demonstrated that these notions and the characteristics concerned are consequences of the general definition of systems or of certain system conditions. In this way, many pseudo-problems which have caused endless discussions and which supposedly trespass the limit of science, disappear in logico-mathematical analysis.

It is a striking phenomenon, the significance of which is hardly understood, that laws which are structurally identical or isomorphic apply to fundamentally different fields. For example, the exponential law applies to radioactive decay, to the breakdown of a chemical compound in monomolecular reaction, to the death of bacteria under the influence of light or disinfectants, to the consumption of an animal by starvation, and to the decrease of an animal or human population where death rate is higher than birth rate. It is equally basic for the principle of diminishing returns, i. e., the fact that in many phenomena the per unit effect of a causal factor decreases with increasing magnitude of that factor. This applies to the relation, in domestic animals, of meat, milk, and egg production to food consumption, as well as to the stimulus-sensation ratio in Weber-Fechner's law. With a positive exponent, the exponential law applies to the unlimited Malthusian growth of bacterial, animal or human populations, to the increase of human knowledge, as measured by the number of pages devoted to scientific discoveries in a textbook on the history of science (18), or by the number of publications on the genetics of drosophila in the last few decades (14). The entities concerned —atoms, molecules, bacteria, animals, human beings, or books—are widely different, and so are the causal mechanisms involved. There is no reduction of the laws of higher levels to physics in the sense of the classical mechanistic view. But the mathematical expressions are identical for all these phenomena. Another equation, the logistic, represents in physical chemistry the expression for autocatalytic reactions; in biology it describes certain cases of organic growth; it was first stated by Verhulst about 1820, and re-discovered by Pearl, for describing in demography the laws of growth of populations in a limited space of living. Equally it governs the advancement of technical inventions such as the expansion of the railroad network in the United States or the increase of the number of wireless sets in operation. The same system of simultaneous differential equations represents in physical chemistry the general expression of the law of mass action, and in demography the general expression for the interaction of species, wherefrom the laws of the struggle for existence

and of biological equilibria between groups of organisms can be derived; essentially the same equations can be used to develop a theory of competition and regulation within the individual organism (21). What is known, in national economy, as Pareto's law (19) of the distribution of income within a nation, and is a general expression for competition, represents in biology the law of allometric growth describing the relative increase of organs, chemical compounds or physiological activities with respect to body size. In biology, the power formula was rediscovered by Lapicque in 1907, and then by J. Huxley in the twenties. A curve describing the growth of certain animals can equally be used for the burden of taxation (personal communication by Professor W. A. Joehr, St. Gallen). Volterra has developed a population dynamics which is isomorphic to mechanical dynamics, and uses concepts such as demographic energy and potential, life action, etc.; it leads to a principle of minimum vital action corresponding to the principle of minimum action in mechanics.

Isomorphies are basic for the use of models and model conceptions in science. The use of models in metal or wood in technology and the abundance of model-conceptions in physics is commonplace. Actually, the progress of science is largely based upon suitable model conceptions. General System Theory will be an important means to facilitate and to control the application of model-conceptions and the transfer of principles from one realm to another. It will no longer be necessary to duplicate or triplicate the discovery of the same principles in different fields isolated from each other.

The isomorphy of laws in different fields has an important bearing on the question of the Unity of Science. So far unification of science has been seen in the reduction of all sciences to physics, in the final resolution of all phenomena into physical events. This could be achieved, if ever, only in an inscrutable future. From our point of view, Unity of Science wins a much more concrete aspect. We are certainly able to state scientific laws for the different levels of strata of reality. And here we find, speaking in the "formal" mode (to use Carnap's (10) expression), a correspondence or isomorphy of laws and conceptual schemes in different fields. Speaking in "material" language, this means that the world shows a structural uniformity manifesting itself by isomorphic traces of order in its different levels or realms. The Unity of Science and the unitary conception of the world is based not upon the possibly futile and certainly far-fetched hope finally to reduce all levels of reality to the lowest level, but is based on the structural isomorphy of laws in the different fields of science and reality. The shortcomings of the mechan-

istic conception have led to vitalism, that is, to the opinion that certain fields are beyond the limits of and inexplicable by exact science. The system conception seems to be an important step toward the mathematization of non-physical realms and their development into exact science.

We have emphasized the usefulness and necessity of model conceptions in science, but they also bear serious danger.

We can distinguish three types of interpretation:

The first level is represented by analogies, that is, apparent similarities of phenomena which correspond neither in the active factors nor in the governing laws. To this class belong the *simulacra vitae,* much in vogue some decades ago, such as the artificial ameba which is a droplet of chloroform wandering about in a dish of water and eating shellac, or the osmotic vegetations of copper ferrocyanate that strikingly resemble a jungle of algae, and so forth. Analogies of this kind have led to the belief that the "riddle of life" is almost solved, that artificial organisms will be manufactured just the day after to-morrow—but, in fact, there is only a superficial similarity with certain biological phenomena.

A second level may be called logical homologies. In this case phenomena differ in the factors involved, but are governed by laws that are of an identical structure or are isomorphic. Such models are widely used in physics. For instance, the flow of a liquid and heat transmission are expressed by the same law, but the physicist is well aware of the fact that there is not a flow of a "heat fluid" but only an imparting of molecular movement. The general system laws of which we are speaking belong to this class of logical homologies.

The third level, finally, is explanation in proper sense, namely, the indication of the actual conditions and factors as well as of the specific laws of phenomena.

We have already mentioned that logical homologies or model-conceptions are one of the most potent tools in science. It will be a further important task of System Theory to control the application of model conceptions and to distinguish exactly between true homologies, i. e., isomorphic structure of laws, and misleading analogies. Take, for instance, the conception of social units, economies, nations, states or civilization, as "organisms." It can be stated correctly that social units are "systems," that is, complexes of elements in interaction to which certain system laws can be applied, a few of which we have mentioned. But System Theory easily shows that these units are not "organisms." The

concept of organism involves further determinations, such as intimacy of the relations between the component elements, centralization, the existence of controlling parts, etc. The conception of social units as organisms is what Vaihinger called a "personificative fiction." It is harmless enough, though not correct if applied to bee-hives or ant communities; in human society, however, it paves the way to serfdom, to use Hayek's (13) expression, and makes the state or the nation a Hobbes' Leviathan, degrading the individual to a mere cell of the social organism. We are organismic philosophers but we remember what Nietzsche has stated as the philosopher's first obligation: namely, to warn against himself.

We are giving now a few examples in order to show how System Theory elucidates age-old problems in science and philosophy.

It is a basic characteristic of every organic system that it maintains itself in a perpetual change of its components. In the cell, there is a continuous destruction of chemical materials through which it persists as a whole. In the multicellular organism, cells are dying and are replaced by new ones, but it maintains itself as a whole. In the biocoenosis and the species, individuals die and others are born. And so every organic system appears stationary if considered from a certain point of view. But what seems to be a persistent entity at a certain level is maintained, in fact, by a perpetual change, building up and breaking down of systems of the next lower order,—chemical compounds in the cell, of cells in the multicellular organism, of individuals in ecological systems.

The characteristic state of the living organism is that of an open system. We call a system closed if no materials enter or leave it. It is open if there is inflow and outflow and therefore change of the component materials.

The characterization of organisms as systems in dynamic equilibrium is old enough but only in recent years the kinetics and thermodynamics of open systems have been developed. It is an expansion of current physical theory, and leads to results that are highly significant from the standpoint of science as well as of philosophy. Only a few consequences shall be indicated here (5).

A profound difference between most inanimate and living systems can be expressed by the concept of equifinality. In most physical systems the final state is determined by the initial conditions. Take, for instance, the motion in a planetary system where the positions at a time t are determined by those at a time t_0, or a chemical equilibrium where the final concentrations depend on the initial ones. If either the initial conditions or the process is modified, the final state is changed.

Vital phenomena show a different behavior. Here, to a wide extent, the final state can be reached from different initial conditions and in different ways. Such behavior we call equifinal. Thus, for instance, the same final result, namely, a normal larva of the sea-urchin will be developed from a complete embryo, from a half embryo after the experimental separation of the cells, from two embryos after their fusion, or after a translocation of the blastomeres. This experiment was considered by Driesch to be the main proof of vitalism. According to Driesch such behavior is inexplicable in physico-chemical terms and can be accomplished only by the action of a vitalistic factor, entelechy, which governs the processes in foresight of the goal to be reached. It is therefore an important question whether equifinality is a proof of vitalism. The answer is that it is not.

Analysis shows that closed systems cannot behave as equifinal. This is the reason why equifinality is found in inanimate nature only in rather exceptional cases. Open systems, on the other hand, where materials are exchanging with the environment, may attain a steady state which is independent of the initial conditions and is so equifinal.

There are certain biological cases where equifinality can be formulated quantitatively, as e. g., in organic growth (7, 8, 9). The same final size which is characteristic for the species can be reached from different initial sizes (e. g., in litters of a different number of individuals) or after a temporary suppression of growth (e. g., by a diet insufficient in quantity or in vitamins). According to quantitative theory, the growth of an organism can be considered as the result of a counteraction between the anabolism and catabolism of building materials. In the most common type of animal growth, anabolism is a surface function, catabolism a function of body mass. With increasing size, the surface-volume ratio is shifted in disfavour of surface. This eventually leads to a balance between anabolism and catabolism which is independent of the initial size and depends only on the species-specific ratio of the constants of metabolism. Therefore it is equifinal.

There is another striking contrast between inanimate and animate nature. In the physical world there seems to be a tendency towards maximum disorder and a chaotic state. According to the second law of thermodynamics, higher forms of energy, such as mechanical and chemical energy, radiation, etc., are continually degraded to indirected heat movement. Heat gradients, in turn, continually disappear, and so the Universe runs down and approaches entropy death as its irrevocable fate when all energy is converted into heat of low temperature and the world process

comes to a stop. In contrast to this, there appears to be a tendency towards states of higher order and differentiation in organic development and evolution. In the billions of years of phylogenetic evolution, as well as in the short span of ontogenetic development, life rises to states of ever higher organization. It has often been stated that a trend towards increasing complication is a primary characteristic of life, in contrast to the inanimate nature. By Woltereck (25) this was called anamorphosis.

These problems gain new aspects if we pass from closed systems, which alone are taken into account by classical thermodynamics, to open systems. The direction of happenings in closed systems is towards states of maximum entropy, because entropy must increase in all irreversible processes according to the second law of thermodynamics. However, in the evolution of open systems, entropy may decrease because there is not only production of entropy due to irreversible processes but also transport of entropy, negative or positive, through the introduction of material from outside. Therefore, such systems may spontaneously develop towards greater heterogeneity and complexity (20). Probably it is just this thermodynamical characteristic of organisms as open systems which is at the basis of the apparent contrast of catamorphosis in inorganic, and anamorphosis in living, nature.

In this way, just the peculiar and supposedly vitalistic characteristics of biological phenomena take on a new appearance in the theory of open systems. We have already stated in 1929 (1) the principles of maintenance in a steady state and that of transition towards higher complication as general characteristics of the living world. Exactly these characteristics have their fulcrum in the kinetics and thermodynamics of open systems. Equifinality, which was brought forward by Driesch as the "first proof" of vitalism, is a consequence of steady state conditions. Similarly, self-regulation in metabolism was considered as explicable only by a governing entelechy, but its general principles follow from the laws of steady states. Anamorphosis contradicts classical thermodynamics, but is in accordance with thermodynamics of open systems. Self-multiplication of elementary biological units, such as genes and chromosomes, was offered by Driesch as a "second proof" of vitalism. If a hypothesis which was advanced by the author (2) should be correct, this phenomenon would also be a consequence of the fact that these units are metabolizing systems. Certainly we do not go far astray if we suppose that the principles of open systems are near to the very root of the central biological problems.

Unified science is an age-old dream, which has found its expression in

Leibniz' idea of *Mathesis Universalis* and in the demonstration *more geometrico* of the rationalist philosophers. It might be that future development of General System Theory will prove to be a major step towards this goal. The mechanistic world view has ushered the world into the unruled domination of physical technology, the mechanization of mankind and the catastrophic crises of our times. An organismic view may lead to a vaster synthesis and a better adjustment to the problems with which we are confronted—provided that we are aware that such a view also has its limitations, and that all our intellectual schemes are but a humble effort to re-draw a few traces of the great plan of reality.

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GENERAL SYSTEM THEORY: A NEW APPROACH TO UNITY OF SCIENCE

2. GENERAL SYSTEM THEORY AND THE UNITY OF SCIENCE

BY CARL G. HEMPEL

Department of Philosophy, Yale University

IN THE present paper, I propose to discuss some of the basic ideas in von Bertalanffy's challenging and suggestive conception of a general theory of systems (1-3), and to examine their significance for the problem of the unity of science.

1. Let me begin by asking what exactly is meant by the concept of system in the proposed theory. In his paper, von Bertalanffy refers to systems as "complexes of elements in interaction to which certain system laws can be applied." Now, this characterization is not circular if an independent definition of system laws is forthcoming. I have not been able to find in von Bertalanffy's writings on the subject a fully general and explicit definition of this concept; but he certainly has provided a great variety of particular illustrations, and has also characterized certain general types of system laws.

One type repeatedly discussed by von Bertalanffy concerns a complex physical object consisting of several parts, say, $p_1, p_2, \cdots, p_n$, which are considered in regard to some quantitative characteristic Q, such as their temperature, or their electric charge, or the concentration in which they contain some specified substance. The values of Q for the different parts, which we shall call $Q_1, Q_2, \cdots, Q_n$, may vary in the course of time; and in particular, it may be the case that what happens (in regard to Q) in one part of the total complex, depends on conditions in all other parts in the following precise sense: the rate at which Q changes in any one of the parts at a given time is a specific function of the values of Q in all the parts at that time. If this is the case,

there exists an interdependence between the parts which can be expressed by a system of simultaneous differential equations:

$$\frac{dQi}{dt} = fi(Q_1, Q_2, \cdots, Q_n)\,;\; i = 1, 2, \cdots, n.$$

This type of interdependence is illustrative of von Bertalanffy's concept of system law (3), but it cannot serve as a general definiens for the latter since in various biological and even physico-chemical contexts much more general forms of interdependence will have to be assumed, and von Bertalanffy would undoubtedly want to include these in the scope of system theory.

It should perhaps be pointed out that a system of differential equations of the kind just characterized actually can serve to determine a *relative concept of system* only, or more explicitly, a *concept of system in regard to a specific quantitative characteristic* Q; and clearly, a physical object consisting of several parts might well constitute, in the sense of the above criteria, a system in regard to some characteristics (e. g., temperature) but not in regard to others (e. g., mass).

Now the task of general system theory would presumably consist in a study of various mathematical forms of interdependence between the constituent parts of a physical object, in regard to one or more quantitative characteristics $Q, Q', Q'', \cdots$. And since we cannot know *a priori* what particular forms of interdependence may prove important in scientific research, a truly general theory of systems would have to investigate, it seems to me, all possible mathematical types of functional interrelationship. Hence, general system theory would coincide with at least the entire mathematical theory of real and complex variables. From von Bertalanffy's writings, I gather that he would hardly want to consider every conceivable type of functional relationship as representing some potential form of synholistic interdependence, and that he would rather construe system laws as some proper subclass of all possible functional relationships. However, he has not, to my knowledge, offered any general criterion to characterize the intended subclass, nor is such a criterion obvious; hence, the meaning and scope of his concept of system remains, to some extent, problematic.[1]

2. But whether system laws are identified with all or only some of

[1] For some suggestive considerations concerning a precise definition of a whole whose parts are interdependent, *cf.* (7).

all possible functional interrelationships, at any rate, general system theory would constitute a branch of pure mathematics.

As a consequence, unlike the theories in physics, chemistry or biology, it would not have any predictive force: its principles would not enable us to foretell the characteristics of concrete systems as yet unexplored. Thus, e. g., in the study of structural similarities between the laws governing different classes of phenomena, system theory could not predict "new" isomorphisms: it would have to wait for empirical research to establish general laws for a new field, and it could then examine those laws for structural similarities with previously accepted ones.

3. What can be accomplished by this establishment of isomorphisms, in terms of which von Bertalanffy proposes to re-interpret the idea of the unity of science? At least this much, to be sure: If the basic laws for a "new" field are isomorphic with those of another, "old," field of inquiry, then we are entitled to assert, without renewed proof, that all the derivative principles of the old field have isomorphic counterparts in the new field. Thus, along with the structure of the basic laws, that of the derivative principles may be transferred from the old to the new context; and this may make for considerable theoretical economy in the sense of Ernst Mach.

It does not seem to me, however, that the recognition of isomorphisms between laws adds to, or deepens, our theoretical understanding of the phenomena in the two fields concerned; for such understanding is accomplished by subsuming the phenomena under general laws or theories; and the applicability of a certain set of theoretical principles to a given class of phenomena can be ascertained only by empirical research, not through pure system theory. Thus, e. g., the realization that Newton's law of gravitation and Coulomb's law of magnetic attraction are isomorphic may give us a feeling of familiarity about one of them if we are already familiar with the other; but apart from this purely psychological consideration, which is irrelevant to the objectives of scientific explanation, the recognition of the isomorphisms adds nothing to that degree of theoretical understanding of gravitational or magnetic phenomena that is afforded by the two laws themselves; for theoretical understanding is reflected in the ability to predict, and the latter obviously remains unchanged by the recognition of the isomorphism.

In discussing the similarities between different types of phenomena and their explanatory value, von Bertalanffy discriminates between mere

analogies and true homologies as based on isomorphisms between the relevant laws. But while such a distinction may be heuristically very useful, the difference on which it rests seems to me a matter of degree. Thus, e. g., the "analogical" similarities between biological phenomena and the *simulacra vitae* consist in certain common behavior patterns; these are not very extensive, to be sure, but they can be expressed in the form of regularities or "laws" of limited scope and precision. Hence, they seem to me to constitute a case of isomorphism which differs from that underlying the "homology" between heat flow and the flow of a liquid only in degree, namely, in regard to the number, scope and quantitative precision of the isomorphic laws involved.

4. In addition to the study of isomorphisms, von Bertalanffy mentions a number of further types of investigation which he considers as representative of the system-theoretical approach. These include his work in the theoretical analysis of organic growth as well as the study of dynamic equilibria and generally, the development of the thermodynamics of open systems. Theoretical research of this kind seems to me to be of great significance for the progress of science; but the results which have been obtained here, or which are obtainable by similar methods, surely are not derivable just from some set of general principles of system theory; rather, each problem in this area demands specific ingenuity and inventiveness: some of them—such as the theoretical study of growth and homeostasis—call for the construction of more or less complex theoretical models which function according to the laws of contemporary physics and chemistry, while others—such as the exploration of the thermodynamics of open systems—even require an expansion of existing physico-chemical theory itself.

But while system theory surely cannot supply us with a set of overall principles which deductively imply the solution to all those special problems, it is well possible that the general mode of approach advocated by von Bertalanffy will prove a highly useful heuristic guide in the search for the solution of specific theoretical problems.

5. I find myself in wholehearted agreement with von Bertalanffy's rejection of the conception of vital force, which has been introduced by neovitalism in an effort to account for certain biological phenomena. But I think that idea has to be repudiated on even more fundamental grounds than that the arguments presented in its favor are inconclusive. The entire so-called theory of vital forces or entelechies is set up in such

a way as to preclude the possibility of any scientific test and in particular of predictions of "new" phenomena. Attribution of some biological occurrence to the influence of a vital force must always have the character of hindsight, since neo-vitalism does not offer any general laws of entelechy which have predictive force in regard to observable phenomena.

6. A rejection of neo-vitalism does not, of course, constitute an endorsement of its traditional counterpart, the so-called mechanistic view; and indeed, if mechanism is construed as the view that organisms are machines in the narrow sense of the word, i. e., that their entire behavior can be accounted for in terms of the laws of mechanics, the shortcomings of mechanism are so obvious as to require no elaboration. But there is a more interesting broader view to which the term "mechanism"—though now somewhat of a misnomer—is frequently applied, namely, the idea that biology, and similarly psychology and the social sciences, can be "reduced" not to mechanics, but to physics in general. And it is clearly to this idea that von Bertalanffy opposes his conception of the autonomy of laws in the different fields of experience. Evidently, mechanism in this broader sense is closely related to physicalism, or the physicalistic conception of the unity of science (6); and with this connection in mind, I should like to consider briefly the question of the reducibility of empirical science, including biology, to physics.

The so-called reduction of one scientific discipline to another may be understood in two major different ways: either as referring to the concepts, or as referring to the theoretical principles, or briefly, the laws, of the disciplines involved. Thus, e. g., in discussing the reducibility of biology to physics, we have to distinguish two problems: namely, (a) the question whether the concepts of biology can all be reduced—in a sense to be examined presently—to those of physics, and (b) whether all the general laws of biology can be derived from the laws of physics. Let us now consider these two aspects of reducibility in turn.

7. In its original form, the physicalistic thesis asserted that all the terms of empirical science (except for those which belong to the vocabulary of logic or of pure mathematics) can be explicitly defined by means of a narrow class of terms taken from the language of physics, namely, by so-called observation predicates. These are terms such as "blue," "hard," "liquid," "coincident with," "between," etc., which refer to directly observable properties or relations of physical objects (in contra-

distinction to terms such as "monovalent," "haploid," etc., which refer to not directly observable characteristics).

The reason offered for that thesis was, briefly, the consideration that even the most abstract concepts of science require rules or criteria for their application to specific contexts, and that these rules must ultimately be expressed by reference to the directly observable aspects of the subject matter at hand—and hence must permit formulation in terms of observation predicates.

This argument, however, overlooked the possibility that those rules of application might not constitute full explicit definitions of the terms in question; and indeed it now seems quite certain that in its narrow original form, the reducibility thesis is untenable. The most general, and decisive, reason I can offer for this claim lies in the consideration that observation predicates have to be non-quantitative in character, and that therefore a finite number of them permit no more than a finite number of conceptual distinctions, i. e., only a finite number of ways in which objects or events can be theoretically differentiated; whereas any one quantitative term as used in scientific theory—say "length in cm" or "temperature in degrees C"—allows for an infinite number of different numerical values and thus offers the possibility of distinguishing conceptually an infinity of different kinds of objects or events. Hence, it is impossible that any of the quantitative terms used in the theoretical parts of empirical science—including the quantitative terms of physics itself!—should be explicitly definable by means of a finite set of non-quantitative observation terms.

This argument applies to the narrow form of so-called operationism as well. Insistence on explicit definability of all legitimate scientific terms by reference to "operations" would force a rejection of the most fruitful concepts in the advanced theories of empirical science. None of the theoretical constructs in terms of which the fundamental principles of such theories are stated can be explicitly defined by means of the directly ascertainable aspects and results of testing "operations."

In fact, the narrow form of physicalism has been largely abandoned in contemporary empiricism, especially since Carnap, in 1936-1937, published his essay *Testability and Meaning*. In that essay, a liberalized version of physicalism is advocated which asserts, in effect, that every term of empirical science can be reduced to a set of observation terms by means of a chain of partial, or conditional, definitions of a specified type, which Carnap calls reduction sentences (for a brief account, *cf.*

(6); for a full theory, *cf.* (5)). While this new idea has shed much light on the structure of scientific concept formation and has provided a valuable re-interpretation of the notion of operational definition, I think that even the reducibility thesis is too strong. For, as closer analysis shows, a chain of reduction sentences for a given term provides a necessary and a sufficient condition for the applicability of that term. These conditions do not, in general, coincide (otherwise, they would provide an explicit definition), but according to the liberalized version of physicalism, they would both be expressible in terms of observation predicates. But there are concepts in theoretical science for which no sufficient conditions can be formulated by means of non-quantitative observation terms; the concepts of having a charge of exactly one elementary electric quantum, or of having a length of exactly 2π cm, are examples. Hence, it appears that even in its liberalized form, the physicalistic thesis is too restrictive.

Let us abandon, therefore, the limitation to non-quantitative observation predicates as the basis for the intended definition or reduction, and let us turn to the more general question whether the total vocabulary of physics, including both observational and theoretical, and both quantitative and non-quantitative terms, affords an adequate basis for the definition of all those terms of empirical science which do not belong to the vocabulary of logic or mathematics.

To begin with a familiar example: It is often said that at present, all the concepts of chemistry are definable by means of physical concepts. Thus, e. g., Russell (9) asserts that copper is definable as the element whose atomic number is 29, and that all the chemical elements can be defined in terms of the elementary particles provided for in contemporary physical theory. Similar claims are often made for the concept of valence and many others. Brief reflection shows, however, that these alleged definitions have the character not of explications of meaning, but of hypothetical laws which provide a micro-theoretical explanation for the occurrence of those particular characteristics by means of which chemical elements are originally identified. Indeed, if the meaning of the term "copper" were given by the phrase "the element with atomic number 29," then it would have been impossible for chemists to use the term prior to the development of modern atomic theory. Those alleged definitions are therefore, at least originally, scientific hypotheses in the form of equivalences as illustrated by the sentence: "A substance is copper if and only if it is the element of the atomic number 29." I said

"at least originally" because it is possible, after the empirical establishment of a law of this kind, to reformulate the theoretical system under consideration in such a way that the equivalence in question acquires the status of a definition. But this process simply shifts the empirical content to another place, as it were; it now becomes an empirical fact that copper as defined in the reorganized system always possesses those physical and chemical attributes which originally served as identifying characteristics for it (*cf.* (8)).

As this brief discussion indicates, the thesis of the definability of all scientific terms by means of the vocabulary of physics will have to be restated as follows: It is possible to reformulate the total set of theories within empirical science in such a way that in the new theoretical system, every non-physical term is explicitly defined by means of a set of physical terms. But this thesis implies that there exists, for every term of empirical science—say "gamete," "inhibition," "democracy"—a set of necessary and sufficient conditions which can be described in physical terms alone. It seems clear that such a claim is not warranted on the basis of present-day scientific knowledge. The thesis under discussion would, therefore, have to be understood as referring to a future stage in the development of empirical science. But in this case, a decision as to its soundness has to wait for the results of further empirical research; it cannot be validated on *a priori* grounds, by means of logical analysis alone. Furthermore, it seems to me worth noting that in this interpretation, the thesis suffers from an unavoidable vagueness. As scientific research and theorizing progress, new terms are going to be introduced; and it is by no means certain that each of these terms will be readily classifiable as physical or non-physical. At any rate, no general criterion for such classification of new terms has been set up, and I doubt that a satisfactory criterion could at all be formulated. As a consequence, the thesis that in a future stage of the development of science, the concepts of physics will suffice to determine all other scientific concepts has no precise meaning.

8. Finally, let us consider briefly the issue of the reducibility of laws and theories, i. e., the question whether all the laws of empirical science are derivable from those of physics. The answer would evidently be in the affirmative if all the terms of empirical science were definable by means of the vocabulary of physics. But as long as the latter thesis is problematic, a law containing, say, certain biological terms cannot,

in general, be logically derived from a set of physical laws, which contain no biological terms at all; rather, such a derivation would require an additional premise in the form of some law or laws connecting the biological concept mentioned in the conclusion with the physical concepts mentioned in the physical laws constituting the other premises. But those connecting laws are not purely physical in character. And even if, by a subsequent reorganization of the system of scientific theories, they should be turned into definitions, and then incorporated into physics, the initial establishment of those connecting laws is a matter of empirical research; whether, and to what extent such interconnections exist, is a question that cannot be settled by logical analysis alone.

But if, for this reason, the reducibility of the laws of the non-physical sciences to those of physics is an open question, so is, of course, von Bertalanffy's opposing view of the autonomy of the laws in different fields of experience. The issue cannot be settled on *a priori* grounds or with philosophical methods alone; it calls for further scientific research.

And furthermore, the very meanings of the opposing views become unclear since they must be construed as referring to a future stage of scientific research, and we have no general criteria by means of which the scientific laws of the future can be unambiguously classified as physical or as non-physical.

It appears advisable, therefore, to construe physicalism as well as mechanism in its broader sense not as theses, but as heuristic maxims for scientific research. And in this capacity, as a challenge to continue and extend the physical or physico-chemical analysis of biological and other phenomena, these doctrines seem to me to have proved highly fruitful indeed.

Certainly they cannot be rejected on charges such as that "the mechanistic world view has ushered the world into the unruled domination of physical technology, the mechanization of mankind and the catastrophic crises of our times." In particular, the mechanistic, or better, physicalistic, program as outlined above does not, of course, assume or imply that men are mere machines or automata, and it provides no ideological justification for the "mechanization of mankind"; it aims at using as far as possible the concepts and theories of the physical sciences to describe and explain, among other things, the various aspects of human experience and behavior—not to deny them or explain them away.

The extent to which a physicalistic analysis of empirical phenomena

is possible can be determined only by further scientific inquiry. At any rate, it seems that a sound physicalistic interpretation of a given discipline can be expected at best when the latter has reached a fairly advanced theoretical stage; and unquestionably, there are at present large and important areas of research—for instance, in the social sciences—where an attempt at description and explanation in physical terms would be at least premature and would be a hindrance rather than a help for scientific progress. In these areas, it is much more promising to attempt the development of appropriate theoretical concepts and principles without any thought of their possible physico-chemical interpretation. And if we construe von Bertalanffy's insistence on the autonomy of different areas of experience rather in terms of this relative autonomy of concepts and principles at least in the initial stages of the development of a scientific discipline, then this idea, as well as the general methodological principles of the system-theoretical approach will, I think, prove to be sound and useful heuristic guides in further scientific research.

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GENERAL SYSTEM THEORY: A NEW APPROACH TO UNITY OF SCIENCE

3. UNITY OF NATURE

BY ROBERT E. BASS
University of Toledo, Toledo, Ohio

THE world of life shows the features of *organization.* For instance, a single green plant has, at the same time, roots for taking in water from the soil, and leaves for assimilating carbon dioxide from the air. Owing to the structure and function of its *organs* the plant can maintain its individual existence. A species of organisms can preserve its life by virtue of the structure and function of the sex organs in simultaneously existing male and female creatures and their instincts and behaviour. Plants, animals, men, all breathe by inhaling oxygen and giving off carbon dioxide. If the atmosphere contained too much of the latter gas, all living things would suffocate. Green plants, however, build up their body by assimilating carbon dioxide from the air and returning oxygen to the atmosphere. Without this process, photosynthesis, we could not breathe. And our breathing supplies the green plants with the carbon dioxide they need for their growth. The world of life on this planet, as a whole, can exist in consequence of the simultaneous functioning of two distinct processes, viz., photosynthesis in green plants and respiration in all higher organisms. These examples show one particular feature of biological organization: Simultaneously existing things have such an order that certain systems can develop and continue to exist.

Having in mind this particular feature of organization, we find it to hold in *physical* nature as well as in life. When we throw a stone, gravity forces it to fall back on the earth. Similarly, it would be possible that the planets fell upon the sun or on each other. On the other hand, some comets first approach the sun, but will later escape from the solar system forever. From the existence of the force of gravity alone we cannot conclude whether the earth will maintain its stable orbit or will

behave like a falling stone or like a comet. Even a precise mathematical formulation of the law of gravity (according to Newton or Einstein) will not enable us to draw such a conclusion unless we know something in addition to the law of gravity, something which is not contained in that law and does not follow from that law in any way. We have to know the simultaneous places and velocities of the earth and the planets at some particular instant. From this "initial condition" and from the law of gravity we can predict whether or not the system is fit for continued existence. In fact, the simultaneous places and velocities of the heavenly bodies *are* connected by such mathematical relations that the continued existence of the system of sun and planets follows. Simultaneously existing things have such an order that the maintenance of the solar system is possible. The common atoms and molecules, the solar system, star systems, and the sidereal universe are fit for continued existence. Physical nature has the property of organization.

Physical nature and the world of life together show the traits of organization. The sun radiates the light required for photosynthesis and building of carbohydrates in green plants. The soil supplies the water necessary for the synthesis of these starches. The sun, the soil with its minerals and water, the gases of the atmosphere, the green plants and all other living organisms together, form a system with such simultaneous properties that the world of life can maintain its existence.

Bertalanffy (4) calls for a universal doctrine of "wholeness and organization." Such a doctrine is possible and, indeed, necessary, because nature exhibits the *fact* of organization. We recognize organized units, namely, single organisms, species of organisms, life as a whole, life and physical nature together. From each unit we progress to the next, more universal one. Organization may be logically classified according to the following scheme:

Organization of Nature

I

Organisms
Physical nature
Organisms and physical nature together

II

Structure of organs and organisms
Functions of organs and organisms
Feeling, perception, instinct, thinking, and behaviour of creatures

III

Preservation of individual organisms
Procreation
Maintenance of life as a whole

In stressing organization, we recognize a property of the cosmos. One essential feature of it has been pointed out: a simultaneous order of objects, in such a manner that systems can develop and continue to exist. Such an order is in nature itself, and it is up to us to recognize it. Can this order be logically reduced to causal laws and probability laws? In my opinion, organization is connected with, but not reducible to, causal laws and probability laws, and is, in this sense, a basic property of the cosmos (1, 2).

Investigating photosynthesis we deal with the sun, its radiation, with chemical elements, with plants, with men and animals. We use the united knowledge of biology, physics and chemistry. Studying organization we achieve unity of science, because nature unites the different spheres, which seem separated to the specialist only if he does not look beyond the borders of his field.

Hempel (6) raises the question for some explicit and precise criterion of "wholeness" in contrast to a "mere sum of physical elements." Considering the mentioned facts, we may designate systems as "wholes," if they show the property of organization. Mere interaction between the parts of a system would not make it a whole, but only such simultaneous configuration and such interaction as will render it fit for continued existence.

Men tried to invent a device which would supply useful work without an input of the necessary driving energy. They failed. But their failure turned out to be a success. From their endeavours and other observations physics has learned a principle: Energy cannot be gained from nothing, nor can energy be destroyed. The energy of a closed system is constant. If the physical universe is such a system, its energy is constant. The theoretical ground-work for this physical principle is most intimately connected with the work of two physicists who were men of medicine, Julius R. Mayer and H. von Helmholtz. Mayer found this principle to be valid in living organisms too. And in many cases the laws of physics and chemistry have been successively applied in biology.

Can we infer from these facts that living organisms are mere physico-chemical systems? The conviction that this is not true is "vitalism," and the assumption that it is the case is "physicalism." The vitalistic thesis as propounded by Driesch (5) says: "Living organisms are auton-

omous." The concept "autonomous" I interpret to mean "not a mere physico-chemical system." The opposing thesis, as formulated by Hempel, claims: "The laws of biology are reducible to the laws of physics." Hempel (6) shows in his analysis that the latter thesis is not warranted on the basis of current scientific knowledge. He encourages, however, further physical and chemical research in biology. Indeed, such achievements have widened our horizon and helped to restore the health of many millions. Fruitful as these endeavours are, do they lead to such conclusions as "the thesis of vitalism is not proved," or "the thesis of vitalism is meaningless"?

Pasteur discovered that germs will be killed by the application of sufficient heat for a sufficient time and that no new life will develop as long as the sterilized substance remains enclosed. Cicero quotes Zeno as saying: "Something which has no life or reason cannot bring forth from itself something which has life or reason." And the belief of the thinkers of antiquity has become the fact of modern science.

Appreciating this fact, we can say: "Life is autonomous." The principle of conservation of energy does not merely mean: "All known trials to construct a perpetual motion machine have failed" for, if just this is said, the possibility is left open that the failure was due to technical imperfections of the apparatus. No, the principle means: "The energy in any closed system is constant." The principle of autonomy of life does *not* merely mean: "All known attempts to develop bacteria from sterilized substances have failed," but it means: "Life is, *essentially*, autonomous." In connection with Pasteur's experiment, this principle is linked with evidence of a similar kind as the evidence for the principle of conservation of energy. Neither principle, however, exhausts its meaning in that evidence. Both principles allow predictions: (1) A machine *will* not (in the future) deliver more energy than has been put in; (2) In a sterilized enclosed substance no bacteria will develop (in the future). The lives of countless patients have been saved by the surgeons' belief in the truth of this prediction.

The machine theory of life and related concepts have been mentioned by our authors. In order to consider these concepts, let us begin with what we positively know. We can feel "I am," "I will," "You are," "We are." A machine can not feel that. We feel joy and sorrow; an automaton does not. Thus the difference between a machine and life is *not* one in degree; it is one in principle. Vitalism is a meaningful conviction.

The laws of wave motion may be investigated by means of mathematics. A set of purely mathematical concepts pertaining to waves may be called a "general system" and the purely mathematical study of waves a "general system theory." (The case of oscillating systems has been considered by von Bertalanffy (3); *cf.* also (4) and (6)). Maxwell's theory of electro-magnetism has discovered radio waves, and Hertz's experiments have shown them. Phenomena of such variety as light, X-rays, and radio waves have been shown by Maxwell's theory and by pertinent experiments to be waves of the same kind. A different phenomenon, sound, has long been known to be caused by mechanical waves. And de Broglie's and Schrödinger's theories and confirming observations have established that even a material system may be considered a wave system. Discoveries showing a "general system theory" as valid in divergent actual cases are significant steps in our recognition of unity of nature, and, therefore, in our striving for unified science.

Unity of science can be based upon unity of nature only. Three avenues of approach have been considered:

(1) The recognition of universal organization;

(2) Discoveries establishing the validity of the laws of physics and chemistry in living organisms; and

(3) Discoveries showing that some general mathematical system theory applies to divergent actual cases.

At the end of their essays von Bertalanffy and Hempel touch upon the reasons for the catastrophic crises of our time.

Unity of nature is not just a fact. It points to a *goal*, to a *higher* unity. The law of nature shows unity, but man does not obey the *moral* law. This failure is the cause of the suffering of the human race. Love from our heart to our fellow creatures will lead us up to the higher unity.

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GENERAL SYSTEM THEORY: A NEW APPROACH TO UNITY OF SCIENCE

4. COMMENT ON GENERAL SYSTEM THEORY

BY HANS JONAS

Carleton College, Ottawa

THREE aspects may be distinguished in von Bertalanffy's paper. While compatible, they are independent of each other, so that the justness, or otherwise, of any one of them does not affect that of the other two.

The first of these is represented by the remarks on the *isomorphism* of laws in different fields of objects, and is signified by the term "general" in the title "General System Theory." We have to examine its alleged bearing on the unity of science.

The second aspect is represented by the remarks on *wholeness*, that concept specifying the wider term "system." Philosophically this is perhaps the most intriguing issue raised, but its scope surpasses the limits of this brief comment.

The third is the *deductive* aspect, more especially the deductive efficacy claimed for axiomatic definition of "system" properties in cases which have hitherto resisted deduction and therefore exact science, such as biological entities and particularly the organism. This aspect, essentially unaffected by the issue of isomorphism and of the unity of science in general, has to be examined on its own merits in that field where it promises new results.

1. To start with, I submit for the consideration of "isomorphism" a very elementary and universal fact. Two apples added to two apples make four apples: two galaxies added to two galaxies make four galaxies: two electrons added to two electrons make four electrons. The parallelism is trivial, though not as utterly as would be the threefold re-assertion of the mere arithemtical fact, $2 + 2 = 4$. Something more is involved. For if we take those statements to refer not to numbers but to numerable reals, and the "plus" as signifying not a mental operation only but an

actual gathering of objects, and consequently the "sum" as expressing their common presence, not just the tautological result of the act of counting, then the statements, beyond reiterating an arithmetical truth, also assert that apples, galaxies, electrons, respectively, are such entities as are capable of forming a sum, i. e., of entering into an assembly in which their individual values, as determinable singly, remain invariant. In other words, they are entities which are capable of simultaneous existence in a common continuum without loss of identity.

Only space, the externally coordinating form of simultaneity, allows such discrete juxtaposition in a common presence. Thus the three statements do carry an ontological implication, albeit a very general one, concerning the kind of being in which their objects share; and it is on this community of being that the "isomorphism" here rests. In Cartesian language, the three classes of objects are equally instances of the *res extensa*. This is a limiting condition for the applicability of the additive law.[1] It follows more generally that unity of science must be grounded in homogeneity of being, and can reach no farther than the latter. In the case of the natural sciences this common ground is provided by the spatial or dimensional character of their objects. This Cartesian reminder, incidentally, implies that I do not think with Hempel that we lack the general criteria by means of which to classify terms as either physical or non-physical.

But once this is understood to be the universe of discourse—as it should be understood in the case of the "General System Theory," which deals with physical entities—the noting in particular cases of the kind of "isomorphism" illustrated by our example is trivial, and does not classify the compared groups in a significant way.

The question before us is whether the isomorphisms to which the General System Theory appeals are, in principle, of that type exemplified by the validity of the additive law in galaxies, apples and atoms, or whether they signify more. For a large group of examples at least, examination affirms the first alternative; others require a more qualified answer.

To the first group I reckon, e. g., the numerous applications of the

[1] Two emotions added to two emotions in one mental state do not necessarily form a sum of four emotions, nor their equivalent in the sense according to which equivalence is verifiable by reference to a common quantitative unit of measurement. Even the sign of equality loses its meaning in the absence of countability and measurability.

exponential and the logistic law. Take, as an example of the former, the parallelism between the decay of radium and the decrease of a population through excess of deaths over births. A curve of the same general form describes the net result of quantitative changes occurring in both complexes. The only condition required for such a curve to result is that the continuous overall change is one in the number of its own agents and that its average rate per unit agent is constant. This condition can be fulfilled in an unlimited number of ways, from the snowball rolling downhill to the multiplication rate of human inventions. For every given case, the applicability of the exponential law is a relevant fact, but is its concordance in that respect with other cases also a relevant fact? It would be, if the holding of the same arithmetical relation were indicative of some common pattern of action whereby it is achieved, and were thus in some way characteristic for the ways or the order of nature. In that case, too, we could draw inferences from an instance where we know the dynamics involved, to one where as yet we only know the overall result in terms of the descriptive curve of change. The situation is well illustrated by our example. In the case of demography, what was known first was the elementary dynamics, viz., the facts of reproduction and death in individuals; then the overall formulae for a multitude were worked out on the assumption of different constant ratios between the two factors, and they showed the general characteristics of the exponential law. With newly discovered radium it was the other way round: first the rate of overall decrease was observed, and the underlying individual dynamics had to be hypothetically supplied. Now in this task the analogy of a decreasing population, far from being helpful, would have been only misleading: it would have suggested to postulate for radium too reproduction besides decay, that very combination being essential for the working of the exponential law in the former case.[2] As a matter of fact, the solution lay in an entirely different hypothetical substructure for the formally identical result: viz., the average proba-

[2] Without new individuals being born against others dying, e. g., in the population of a monastery with no accession of novices, the natural decrease would not go by a constant percentage of the surviving number at each time, but (assuming an even age distribution) by a constant percentage of the initial number, terminating in zero when the youngest member reaches the limit of the human life span. Radium seems to be fairly much in the position of the dying monastery population: liability of all members to natural death, and no reproduction. Yet in the mathematical result it "behaves" like a reproducing population.

bility per unit time of spontaneous decay as a constant, applying to each individual throughout infinity, i. e., without influence of age. We note, incidentally, that in this case not even interaction is involved in procuring the result which, therefore, here does not represent a "system" property. Thus, relevant as is the information that the exponential law does apply in the decay of radium, as irrelevant in that context is the information that it also applies to the decrease of a population (or rather, would apply if the excess ratio of deaths over births remained constant, which as a rule it does not do for any length of time). It may still be argued that the very fact that in so many different ways the same formal overall result emerges is significant in pointing to a structural characteristic of nature. But the fact does really, in the last analysis, not amount to more than, e. g., the fact that 5 may result as well from the addition of 4 to 1, as from the subtraction of 2 from 7, and from any number of additions and subtractions combined. No preference for fiveness is implied in this range of possible concurrence. No unity of science, as far as I can see, is to be built on the discussed kind of isomorphism.

There are other cases where the isomorphism is more specific and thus more significant, but to the same extent narrower in range; and at the other end of the scale von Bertalanffy's own work presents us with equations which, by the particular assumptions made in them, refer in effect to one highly specific class of objects, viz. living organisms. The problem they raise can be quite dissociated from the issue of a General System Theory: it concerns the formalization and mathematization of biological facts. This is a highly interesting attempt in itself; and I cannot help feeling that, by linking its validity with the more general issue, von Bertalanffy does less than justice to his solid results in the special field.

2. The method adopted by von Bertalanffy is to define a type of system through a suitably chosen dynamical property, capable of being formalized in an equation, and hence to evolve series of equations which, when retranslated into the language of experience, show other properties of such a system to follow from the first one, i. e. to be formally inherent in that type of "wholeness." Obviously the working of the method, i. e. its yielding more than tautological results, turns very much on the status and the deductive virtue of axiomatic definition. Here I must briefly take issue with a statement in Hempel's paper which in most other respects strikes me as eminently sound.

That the familiar substance copper turned out to have the atomic number 29 was indeed an empirical discovery, and the empirical equivalence thereby established, so long as it is no more than that, does not lend any intrinsic precedence to this over others of its distinctive characteristics to serve as a definition of the *term* "copper." But once the full implications of atomic number within the theory of matter are realized, it is no longer just an empirical fact that an element of this number possesses all those physical and chemical characteristics which originally served to define it: rather, they now appear to follow demonstrably from that primary property when this is entered as a specifying condition into the general laws of material structure. If so, the definition by this property acquires a singular status as a basis for deduction. It is the definition no longer of an existing term, but of a constructive pattern establishing the possibility of that term, and insofar *a priori*. We may call this a fundamental or constitutive definition, in contrast to a merely descriptive or indicative definition. But, of course, the condition and measure of its deductive power is the availability of the axioms which govern the possible combinations of elementary matter. These can only be ascertained by empirical analysis: in Galileo's language, the range of the "compositive" method depends on the depth reached by the "resolutive" method. This seems to preclude the possibility to base physically relevant deductions on merely mathematical assumptions.

We have to examine whether the logically very different kind of definition which von Bertalanffy applies to organic systems possesses a deductive range and precision comparable to the kind just exemplified. To do this properly would require us to go into the mathematics of the cases so far offered, and this is beyond our present scope. Some observations of principle, however, can be made without becoming too technical.

Organisms are defined as "open systems," and this characteristic, taking into account only the fact of inflow and outflow, can be expressed in a very generalized equation. Nothing, be it noted, is specified concerning the kind of elements involved, nor concerning pattern of arrangement, or acting forces either. The equation, therefore, applies no less to the basin of a Roman fountain (as described in a famous German poem [3]) than to a living cell and to a multicellular organism. One of the several possible solutions of this equation defines the final state of the system as a "steady state" in which inflow and outflow balance each

[3] "*Der römische Brunnen*" by C. F. Meyer.

other; and, with certain assumptions concerning constants, it can be shown that this final state depends only on the system constants, and not on the initial state or the intermediate phases. It may thus be reached on different routes.[4] Now, is the "equifinality" so defined that of Driesch's sea urchins? Obviously only by a stretch of interpretation. To Driesch, the puzzle was morphological, not quantitative (such as an equilibrium of metabolism), and this aspect is not covered by the steady state equation. The effectuality of von Bertalanffy's demonstration would in this case very much depend on Driesch's willingness, which I doubt would be forthcoming, to recognize his fact in the algebraic description, viz., to recognize that what amazed him so much in the outcome of his experiments is properly described as the attainment of a quantitative balance of inflow and outflow. In fact it was the spontaneous self-integration of the organic *form*, as a differentiated whole, against structural lesions of the germ.

Similar observations could be made with respect to such concepts as "centralization," "progressive segregation," and others, which, when referring to organic facts, have a highly morphological meaning, and this the respective formulae do not contain. They all, therefore, leave their application much more tentative, and indeed require for their very interpretation much more previous knowledge of the entities which they might fit, than do the deductions from definitions of the "atomic number" type.

The reason for this gap between formula and fact, which is at variance with the classical type of physical deduction, is that the initial equation of, e. g., an open system, being purely arithmetical, does not contain any elements pertaining to pattern or structure, which, e. g., the expression "atomic number 29" for copper does contain in conjunction with the laws of corpuscular aggregation. In its axiomatic context the latter expression is geometrical as well as arithmetical: therefore a morphological, not only quantitative statement: therefore specifying the con-

[4] Thus each basin of the Roman fountain will finally stabilize its water content by overflow, irrespective of its initial content, and irrespective of how much the cascading from above might have been slowed down or accelerated in the process. The last two lines of the poem describe the "final state" of the "system":

Und jede nimmt und gibt zugleich
Und strömt und ruht.

"And streams and stays" is a perfect description of the "steady state" for which in fact von Bertalanffy introduced in German the term *Fliessgleichgewicht*.

struction of a model and permitting precise deduction. But from merely arithmetical premises, no morphological conclusions derive. Quantity will not yield form.

Furthermore, just as form, so forces are on purpose not specified in the definition of an open system. Thus we do not know from the definition what kind of causality may actuate the metabolizing process, or even whether nature has at all the causal means at her command to realize a system which maintains its metabolism by effecting itself the transport of materials—a striking characteristic of animal life. We know this as an empirical fact and still wonder how nature does it, i. e., what principles of operation are involved. And just as the initial equation only states the formal condition, to be fulfilled by nature if and how she can, so the deduction only states that if it is fulfilled, certain consequences may follow, operated by the same unspecified dynamics. If so, the logical conclusions of the system theory cannot have a bearing on the issue of vitalism vs. mechanism, which is essentially an issue concerning the type of causality involved. I do not wish to go on record as a champion of vitalism —which would indeed be imprudent if vitalism were quite the thing which von Bertalanffy and Hempel condemn in their papers. I simply wish to state that the issue is not touched by the system theory, and is on principle, according to the logical status of that theory, outside its scope. Since nothing is stated of modes of action in the defining equations, nothing concerning them can be elicited from the derivative equations. They still, so far as they themselves go, leave the choice open between mechanical and final causation—which choice, from evidence quite other than the mathematical one, has to be made to fill in the scheme. That the search will by all means go into the former direction (mechanical cause) is part of the scientific imperative rightly pointed out by Hempel; but its result, or possible success, cannot be anticipated by the system concept: it concerns the explanation, not the formulation of the total behaviour. Thus to say of a vital phenomenon that it is a system property and the necessary (more often: the possible) consequence of the defined type of system, is not to say anything about the non-vitalistic nature of the whole.

Questions of fact are not settled by definition. The problem of mechanism or teleology, as also that of wholeness, are (to use the disreputable word) metaphysical and not logical issues. Mathematics cannot decide them, indeed does not pronounce on them at all. Formalization leaves the contents untouched. The merits of the biological system theory,

as actually worked out by von Bertalanffy, lie in its quantification of biological phenomena which allows the formulation of interconnected quantitative laws: these are descriptive, not explanatory, and descriptive of course only of the quantitative aspects of organic facts. This leaves a wide enough scope for the system theory to be a valuable instrument, even if we find it not to be competent to deal with some of the problems which its originator sees included in its scope. A great advance in biological method, it leaves the philosophical problem of organism essentially where it was before.

GENERAL SYSTEM THEORY: A NEW APPROACH TO UNITY OF SCIENCE

5. CONCLUSION

BY LUDWIG VON BERTALANFFY

Department of Biology, University of Ottawa, Ottawa

THE problem which is at stake in the present discussion is a basic one. It is the question whether generalized theoretical constructs are possible which—as compared to the only advanced exact science in existence, namely, theoretical physics—can embrace the other fields of reality, the biological, psychological, sociological, etc., and can deal, in particular, with those aspects of "organization," "wholeness," "teleology," and the like which are outside the so-called mechanistic conception. As has been already stated, there are different attempts in this direction. It is obvious that they are tentative. Their merits or shortcomings cannot be judged on *a priori* grounds, but will become apparent only in concrete application. The present discussion is concerned with General System Theory. However, the arguments proffered are typical, and most of them will apply, *mutatis mutandis*, to any attempt of generalization of scientific concepts. The commentators being philosophers, it may be expected that they have used the acumen of logical and epistomological criticism. The scientist, however, knows that theories in general, and new theoretical attempts in particular, can hardly live up to the standard of explaining ultimate questions, and of that degree of "exactness" the philosopher is likely to expect.

It appears that the need for generalized scientific conceptions is felt in various quarters. An important prerequisite is synthesis and co-operation between different branches, and suitable model conceptions which are general enough to be transferred from one realm to another. This has been demanded recently, under the heading of the "Education of Scientific Generalists," by a group of scientists comprising the engineer Bode, the sociologist Mosteller, the mathematician Tukey, and the biolo-

gist Winsor (5). They emphasize "the need of a simpler, more unified approach to scientific problems," as manifest especially in borderline fields like physical chemistry, biophysics, and the application of chemistry, physics, and mathematics to medicine. To the question, "Who needs scientific generalists?" they answer:

> "Any research group needs a generalist, whether it is an institutional group in a university or a foundation, or an industrial group. . . . In an engineering group, the generalist would naturally be concerned with *system problems. These problems arise whenever parts are made into a balance whole.* In biological and medical sciences, he (the scientific generalist) would provide efficient interpretation of data and design of experiments, and—what is most important—would suggest *physical explanations* or *mathematical models* for known or conjectured facts. The need of such useful and stimulating people is probably better recognized in this field than in any other we know" [italics mine].

In a similar way, Wiener states in his *Cybernetics* (12):

> "There are fields of scientific work which have been explored from the different sides of pure mathematics, statistics, electrical engineering, and neurophysiology; in which every single notion receives a separate name from each group; and in which important work has been triplicated or quadruplicated. . . . It is these boundary regions of science which offer the richest opportunity for the qualified investigator."

It can be easily understood why such need is felt in the various branches. Biology is concerned with organisms, which, by definition, are "organized" things. It appears, therefore, that the first objective of biological theory would be to define what is meant by "organization" in general, and "biological organization" in particular. No such theory exists. There is, of course, an enormous amount of facts concerning organization at its different levels, from the structure of organic compounds through submicroscopical morphology up to cytology, histology, and anatomy. But this information remains descriptive, and it appears that for two reasons, namely, the specificity of biological organization, and the complexity of organic systems (*cf.* 3), a mere application of the concepts of conventional physico-chemistry will not be sufficient, but more general conceptions are necessary. Actually, we find in different fields theoretical constructs which reckon with entities not in the physico-chemical, but in the biological scale.

The situation in psychology is similar. The evolution of psychological

theory and of theory of behavior depends on suitable model conceptions. The limitation of physiological and neurological models—such as the classical neuron scheme, the electrical fields invoked by Koehler to deal with *gestalt* phenomena, etc.—is that we know so little of what actually happens in the brain. It might be that generalized model conceptions applying to the global or " molar " aspects of the activity of the central nervous system and of behavior may prove advantageous as contrasted with models on the " molecular " level trying to explain psychological phenomena in terms of microscopical happenings—nerve paths, chains of neurons, and the like—which, at least at present, must remain largely hypothetical. In this vein, Thumb (11) has welcomed the concepts of steady state and of " dynamic morphology " as suitable model conceptions in psychology. Krech (7) proposes " dynamic systems as open neurological systems." The trend of psychological theory, as exposed recently by Brunswik (6), suggests that generalized models, perhaps in the line of General System Theory, non-committal in respect to too specialized neurological hypotheses, may be the adequate tool for a " molar approach " as demanded in recent research.

Again, in sociology, the need of a theory of organization was stressed by Lienau (8), and recently the problem of a " measure of organization " was discussed at a seminar at Columbia University (9). Possibly such measure of organization can be found in General System Theory in terms of interaction, segregation, centralization, and leading parts. We could imagine that the formulations proposed (4, p. 146 ff.) lend themselves to an interpretation in measurable sociological quantities, this case being easier to handle than the biological organism with its enormous complexity.

These are the issues that are at stake, and it is more important to realize them than to detect flaws in the present, necessarily preliminary, formulations. We have, however, to envisage the criticism proferred, in order to make sure whether scientific generalizations and general model conceptions are not in the wrong from the beginning.

Hempel feels the want of a fully general and explicit definition of the notion " system," though he admits that a great variety of illustrations was provided. This argument is correct. The shortcoming mentioned is partly natural in view of the difficulties of the subject, and is partly a matter of policy to choose the best way of presentation. In a somewhat similar situation, namely, in chemical kinetics, Auerbach (1924, after Skrabal, 10) has pointed out that the " abstract " method which starts

with the most general formulations and derives with full rigor a deductive theory, is doubtless supreme in the last resort. "For a first presentation, however, for elucidating the original darkness, the concrete method" (presenting the issue by way of illustrative examples) is "if not the only possible one, in any way to be preferred because it stimulates much easier resonance and lively interest."

It is understood that more rigorous and at the same time more general formulations have to be given if General System Theory deserves further elaboration. However, it is not a mere catalogue of well-known differential equations and their solutions. This is shown by the fact that it poses definite problems. For example, the question of the general conditions for the existence of stationary states and of periodic fluctuations is far from being solved, except for simple linear cases. A principle of minimum action appears in mechanics, physical chemistry (Le Chatelier's principle), electrodynamics (Lenz' rule), and demography (Volterra). It seems that a generalized principle of minimum action is demanded which is applicable not only to physical systems, but to systems of any kind. A general theory of periodicity is a desideratum in many fields. Problems of relaxation oscillations appear in physics, in neurology, in the theory of biocoenoses, of economic cycles, etc., and a theory generalized with respect to physics, and applicable to any type of system, would doubtless go a long way in exploring these different phenomena.

There is, however, Hempel's objection that a general theory of systems "has no predictive force" although he admits that the transfer to new fields of established principles may make for theoretical economy in Mach's sense. He makes a distinction between General System Theory which "would constitute a branch of pure mathematics" and is "unable to foretell the characteristics of concrete systems as yet unexplored," and the "theories in physics, chemistry, and biology" which can do this job and are "surely not derivable just from some set of principles of system theory." We do not see, however, a fundamental difference. In every hypothetico-deductive system—be it geometry, mechanics, probability theory, or economics—we have to introduce special conditions according to experience in order to apply it to concrete phenomena. The same way can be followed in General System Theory. A good example is the theory of populations, first developed by Lotka, later accomplished by Volterra, Kostitzin, and others: Starting from a general and purely formal set of equations, and gradually introducing more specific conditions, laws for the growth of populations consisting of one or more species, for competition,

biocoenotic equilibria, fluctuations of populations, and so forth, can be derived. Similarly, we can start with a system of simultaneous differential equations, and introduce as a more specific condition that the system shall be "open," i. e. that there shall be inflow and outflow of material. Then we get the general kinetics of open reaction systems. We may further introduce the reaction constants of the chemical compounds concerned, and may use our equations to predict processes in industrial chemistry; or we make certain conditions as to the ratio between inflow and outflow—for example, that the first be proportional to a $^2/_3$ power of mass, the latter to mass itself—and we get growth laws which hold for many organisms, partly allowing very unexpected predictions.

Next we come to the question of the significance of isomorphy. According to Hempel, the isomorphy of laws "adds nothing to the understanding" of the phenomena concerned. He quotes Newton's and Coulomb's law as an example. But this example proves exactly the opposite of what is claimed by Hempel. Far from being considered to be a mere and superficial coincidence, the isomorphy between electrical forces and gravitation is a corner stone of general field theory and relativity theory.

According to Jonas, the occurrence of structurally corresponding laws in different realms is a mere mathematical coincidence, and amounts to no more than the truism that mathematical statements, such as $2 + 2 = 4$, can be applied to apples, atoms or galaxies. This objection is best answered by giving another example. A classical case of Gaussian distribution is the deviation of hits from the bull's eye of a target; on the other hand, a Gaussian curve applies—among innumerable other phenomena—to the distribution of phenotypes in the seemingly non-Mendelian heredity due to multiple factors, for example, length of ears in corn or other differences in size. This example corresponds in its logical structure to that given by Jonas. In the first case, an overall formula covers a directly observed phenomenon. In the second, hypothetical entities are assumed in order to explain an observed phenomenon. Following Jonas' line of argument, we would have to say: in inheritance based upon multiple factors, there is certainly no gun, gunner, or target; it would be nonsensical to apply such notions to the hypothetical substructure of heredity; that the same integral applies in both cases amounts to no more than that $2 + 2 = 4$; and statistics is bunk. Fortunately such a conclusion was not drawn, and so the question remains as to what is at the basis of isomorphy. The answer is, of course, that it is the concept of

chance. In the case of the marksman, the shooting is influenced by a number of independent factors. In the case of multiple factors in heredity, the distribution of phenotypes results from the combination of a series of independent genes that control a character such as size. Therefore, the binomial distribution will apply in both cases.

The mathematical formulations are expressions and consequences of certain conceptual models we apply. One of these models is the world as a game of hazard; based upon this, we apply the laws of probability to physical, biological, and sociological events. Another model is the world as a hierarchy of integrated wholes. Assuming that we have "systems," certain general principles will apply even if the component entities are different. In this sense, the isomorphy of laws is no mere coincidence, but is indicative of a structural uniformity of phenomena and a corresponding unity of science. Of course, the occurrence of identical mathematical expressions is not in itself a proof that the phenomena concerned are "homologous" system phenomena; all that is contended is that if systems are considered, certain general principles will apply to them.

The statement that two planets plus two other planets make four planets is not called a law of nature; but the statement that planets attract each other inversely proportional to their distance is.

There is a clear-cut criterion to decide whether mathematical statements belong to the first or the second type. A mathematical expression is a truism or a tautology if there is no other alternative. For this reason, the statement $2 + 2 = 4$, as applied to apples, atoms or galaxies, is neither a law of nature nor does it entail a theory. Newton's law, on the other hand, is a law owing to the fact that there are other alternatives: it would be quite possible that the force of attraction is inversely proportional not to the square but to some other power of distance. Similarly, as long as only Euclidean geometry was known, the assertion that the sum of angles in some cosmic triangle is 180°, was not a statement of science but a self-evident truism. But when other alternatives, the non-Euclidean geometries, were discovered, space became an issue of physics, and different theories were developed according to whether space is Euclidean or which type of non-Euclidean geometry applies. The same is true of the statements with which system theory is concerned. It is not self-evident that organisms are governed by dynamical laws of interaction. On the contrary, the opposite view was accepted for centuries, namely that the organism is a machine, and that the processes within it are governed by one-way causality. Similarly, it is not self-evident that

the neurological basis of mental phenomena is governed by system laws. The contrary was contended by classical association psychology, and the discovery that this model is not adequate is the achievement of *gestalt* theory. Again, the laws that govern population growth or competition are not self-evident; they had to be discovered, and actually were discovered several times independently. Therefore the statements concerning systems are not mathematical truisms but they involve a theory about the entities concerned. If this is correct, a general theory of the entities called systems is justified.

Jonas' argument is based upon a *quaternio terminorum*. He muddles up two essentially different kinds of statements, namely, those of the type of mathematical tautologies and those of the type of laws of nature; and he draws the wrong conclusion that, because the appearance of identical mathematical expressions is trivial in the first case, the same is true also in the second. Only this mixing-up makes possible his conclusion that the isomorphy of system laws (i. e., of non-tautological statements about entities envisaged under a certain model conception) is of the same kind as the fact that the number 5 may result from any number of additions or subtractions (i. e., mathematical tautologies whose applicability to different entities is trivial).[1]

With respect to the problem of vitalism, Jonas is right in emphasizing that the present formulation of equifinality concerns only certain quantitative and not the morphogenetic aspects proper. The philosopher is in the enviable position of being able to denounce the scientist for not having solved the "problem of life," without having to worry to do a better job. It is not correct that morphological problems are not accessible to arithmetical treatement; there are important beginnings of a theory of organic forms in allometry, in D'Arcy Thompson's theory of transformation and the like. It may be, however, that new forms of non-quantitative or *gestalt* mathematics will be necessary to deal fully and adequately with the problem of organic form.

Such theory, however, will certainly not be reached by declaring these issues "metaphysical." It is true that "explanation" in science means

[1] The case of the exponential law, quoted by Jonas and illustrated in an amusing way by the "monastery" simile of the radium atoms, is taken care of as a "degenerated" form of system (4, p. 147). That the exponential law can result from independent action of the elements (radium decay, decrease of a population where there is no multiplication) as well as from interaction within a system (increase of a population by sexual reproduction) was discussed in (2).

nothing more than deduction of more special from more general statements, and it is thus only a "description" by means of theoretical constructs. We don't know exactly what that "metaphysical" explanation actually means which Jonas demands for the problems of mechanism, teleology, and wholeness, and which supposedly answers the "philosophical problem of organism." Whatever it might mean, we gladly leave it to the philosophers. But since they have been quarreling about these issues for some two thousand years, endlessly repeating exactly the same arguments in slightly changed masquerade and without any visible progress, we have little hope that they will find out in the near future. The scientist is satisfied if he can mirror a few aspects of reality in theoretical constructs, knowing well their limitations, but being able, to a certain extent, to control nature in thought and in practice in this way.

With respect to the question of "reducibility," we do not, as Hempel thinks, contend "autonomy of laws in the different fields of experience" as an opposing view, but fully agree that the question whether and how a reduction can be achieved can be answered only by research itself. It appears that both views which may be called the "reduction thesis" and the "stratification thesis" (N. Hartmann's *Schichtengesetze*) are indispensable maxims of research. The "reduction thesis," asking that higher levels, especially the biological, should be resolved into physico-chemical phenomena and laws, is of course basic in the physico-chemical investigation of biological phenomena, the achievements of which need not be emphasized. The "stratification thesis," on the other hand, warns us that the principles of the higher levels must first be stated as "autonomous" laws, before further reduction can be attempted. Chemistry has not developed by an application of the laws of mechanics or other fields of physics; first a system of autonomous chemical conceptions and laws had to be developed before a fusion of chemistry and atomic physics could take place. In biology, forms of order and organization appear that are specific and unknown in the realm of the non-living; further, the process is so complicated that with respect to phenomena concerning the organism as a whole, the individual physico-chemical events cannot be taken into account, but we have to reckon with units of a higher level and global laws.

The central point of system theory is the dynamic view, trying to explain phenomena of order in terms of the interaction of processes, as contrasted with the Cartesian machine theory, which tries to explain it in terms of pre-established structures. This general model conception,

and not the triviality that differential equations can be used to describe various phenomena, is the meaning of General System Theory. We can express the same also in another way: Besides "machines," the origin of which remained uncertain—Descartes invoked the Creator, modern times Natural Selection—the general model in the mechanistic conception was a world of events at random, whereas in the new conception, the general model is a world of organization. Or, in a third formulation, we have a transition from a "self-actional" viewpoint (conceiving isolated causal chains, processes, egos), over an "inter-actional" (conceiving the interplay within these units) to a "trans-actional" standpoint which acknowledges that there is no sharp borderline between the "knower" and the "known,"—that no actual system is isolated. In this sense, Bentley (1) acknowledges that the concept of "open systems" is a general "trans-actional" one, embracing the relation of behavior in environment as a more special case. There seems to be no doubt that the general trend of modern thought is in such direction. When it was said that the "mechanistic view has ushered the world into the unruled domination of physical technology," it was meant not as a cheap argument against mechanism or in favor of organicism. Rather it is a statement of what seems to be a historical fact, namely, that scientific theories, even of a highly technical and specialistic nature, are expressions of a common mental pattern or *zeitgeist* of the epochs concerned. This is true for the mechanistic view of the 19th century as well as of the synholistic or trans-actional notions of our time. Whether a world governed by such principles will be a better place to live in, we do not know and we have erected conspicuous warning posts against an over-optimistic view.

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GENERAL SYSTEM THEORY: A NEW APPROACH TO UNITY OF SCIENCE

6. TOWARDS A PHYSICAL THEORY OF ORGANIC TELEOLOGY

FEEDBACK AND DYNAMICS

BY LUDWIG VON BERTALANFFY

Department of Biology, University of Ottawa, Ottawa

IN recent years, a new discipline, called Cybernetics, has been founded by the mathematician N. Wiener, which claims to pave the way for a new understanding of the organism (Wiener, 16; *Teleological Mechanisms*: Conference, 15[1]). The general characteristic of the theory is its inter-scientific character: Cybernetics embraces communication engineering, physiology, especially of the nervous system, neuropathology, psychiatry, biocoenology, sociology, epistemology, and other realms, contending that certain model conceptions are applicable in quite different fields. In this respect, it is parallel with General System Theory.

We reproduce in a condensed form, the programmatic statements given by L. K. Frank (Conf.):

> "The concepts of purposive behavior and teleology have long been associated with a mysterious, self-perfecting or goal-seeking capacity or final cause, usually of superhuman or super-natural origin. To move forward to the study of events, scientific thinking had to reject these beliefs in purpose and these concepts of teleological operations for a strictly mechanistic and deterministic view of nature.
>
> "With the new concepts and the more precise methods of investigation and measurement, science made astonishing advances, progressively revealing the order of events without assuming any goal-seeking or purpose-striving—teleological process—in nature. This mechanistic conception became firmly established with the demonstration that the universe was based on the operation of anonymous particles moving at random, in a disorderly fashion, giving rise, by their multiplicity, to order and regularity of a statistical

[1] Quoted here as "Conf."

nature, as in classical physics and gas laws. The unchallenged success of these concepts and methods in physics and astronomy, and, later, in chemistry, gave biology and physiology their major orientation. These disciplines approached problems as essentially similar to physics, as problems of the relation between two variables, to be isolated by a progressive fractionation of organisms into ever smaller parts and the statistical manipulation of data to reveal these relations as well as the mechanisms and factors. This approach to problems of organisms was reinforced by the analytical preoccupation of the Western European culture and languages. The basic assumptions of our traditions and the persistent implications of the language we use almost compel us to approach everything we study as composed of separate, discrete parts or factors which we must try to isolate and identify as potent causes. Hence, we derive our preoccupation with the study of the relation of two variables, to be found by abstracting parts from organisms, as in laboratory preparations.

"Again, the success of these methods has been astonishing, revealing the ever finer structural, functional, and behavioral aspects of organisms. However, within recent years, there has been a growing perplexity in biology and psychology. As they have pursued this analytical method and disclosed ever more subtle and elusive factors and mechanisms, it seems to carry them further and further away from the organisms and personalities they were trying to understand. The inconsistencies and conflicts of theory started a critical examination of basic assumptions and the organizing conceptions and methodologies. Thus, we are witnessing today a search for new approaches, for new and more comprehensive concepts and for methods capable of dealing with the large wholes of organisms and personalities. The term *organism-as-a-whole* has been used, along with *holism*, *synholic*, and similar expressions designed to recognize the patterned, organized structure-function activities, internally and overtly, of living organisms. There also has been growing recognition of the patterning of perception, as in *gestalt* psychology.

"The concept of teleological mechanisms, however it may be expressed in different terms, may be viewed as an attempt to escape from these older mechanistic formulations that now appear inadequate, and to provide new and more fruitful conceptions and more effective methodologies for studying self-regulating processes, self-orientating systems and organisms, and self-directing personalities. But these new concepts carry no psychic or vitalistic assumptions, nor do they imply that any mysterious supernatural powers or psychic forces or final causes are operating the system or guiding the organism-personality. The idea of purposive behavior is not a regressive movement to an earlier stage in the history of ideas, but a forward movement toward a more effective conception of the problems we face today.

"Thus, the terms *feedback*, *servomechanisms*, *circular systems*, and *circular processes* may be viewed as different but equivalent expressions of much the same basic conception. The idea of self-regulation, with goal-seeking behavior, becomes applicable in the laboratory, in the clinic, especially for study of social orders and cultures."

From this may be seen the basic significance credited to these developments which another participant at the Conference (McK. Rioch) declares to be "of historical importance." Secondly, the close correspondence with movements on the European continent is apparent, as may be seen by a comparison of the statements given above with, e. g., a recent presentation by the present author (2). There are many more points where this parallelism is manifest: the opposition, as against the analytical, cause-effect, stimulus-response, and two-variable scheme, of the concept of mutual interaction (*cf.* Conf. p. 194 f., and 2, p. 110 ff.); the statement that there seems to correspond to the principle of complementarity in physics (stating that we cannot simultaneously measure both position and velocity of a particle) a fundamental limitation in biology, namely, a complementarity between investigation of isolated processes and that of the organism-as-a-whole (Conf., p. 193 f., and 2, p. 146 f.); the emphasis laid upon the dynamic, instead of static, character of organic equilibria and the insight that the apparently solid organism is, in fact, a continuous flux (Conf., p. 191, note, p. 194, and 2, chap. 4); based upon this, the overcoming of the ancient antithesis of structure and function since the organism is, in fact, a spatio-temporal system (Conf., p. 194, and 2, p. 128 ff.; 1, I, p. 246 ff.); the re-orientation in the concepts of time and energy (Conf., p. 197 ff., and 3); the realization that, as against the timid belief that the inductive method leads to fundamental theories and laws, the progress in physics was, and in biology may be, achieved by the way of bold theoretical constructions (Conf., p. 195, and 1, I, p. 27 ff.; 1, II, p. IX).

The basic importance of these problems, and also the parallelism with the viewpoints advanced for more than twenty years by the present author, invites a critical study of Cybernetics. In the following, we give a free transcription of the main arguments.

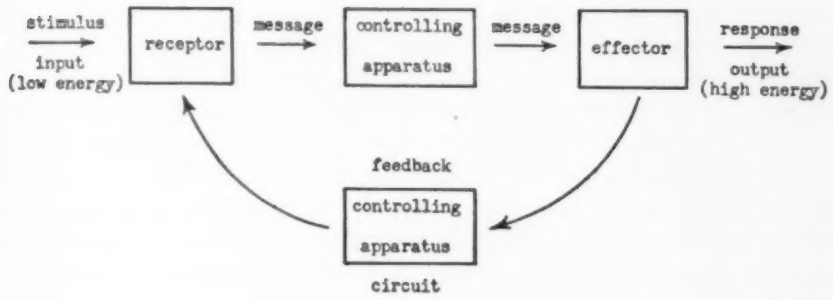
The basic model conception of purposive behavior offered by Cybernetics is that of *feedback mechanisms*. The general principle is that from the output of a machine a certain amount is led back, as "information," to the input so as to regulate the latter and thus to stabilize and direct the action of the machine.

The classical example from which Cybernetics takes its name κυβερνητης—steersman—governor) is the governor of the steam engine, consisting, in its original form as designed by Watts in 1784, of two balls swinging on a rotating shaft and coupled with the engine. They are kept down by their weight, but swing upward according to the

centrifugal force acting. Their position is transmitted to a collar which by an intermediate link opens the intake valves of the cylinder if the engine slows down and the balls fall, and closes them when the engine speeds up and the balls rise. Another example is the thermostat, opening the entrance of fuel if the temperature goes down, and closing it in the opposite case.

The same principle is, of course, widely applied, not only for stabilizing a desired effect, as for instance, in reaction-coupling in radio, but also to direct actions desired by feeding back information, as in ship-steering systems, self-propelled missiles which seek their target. anti-aircraft fire-control systems, and other "servomechanisms."

A simple feedback arrangement may be schematized somewhat like this:



The system comprises, first, a receptor or "sense organ" whether in the flesh or in the metal, be it a photoelectric cell or another receptor for light, a radar system, a thermometer, a microphone, a pH-recorder, or a sense organ in the proper meaning. The message is, in general, characterized by the fact that it is low in energy but high in "information." It may be, in technological devices, a weak current, or, in a living organism, represented by nerve conduction. Then there is a relay recombining the incoming messages and transmitting them to an effector, consisting of a machine like an electromotor, a heating coil or solenoid, or of a muscle which responds to the incoming message in such a way that there is power output of high energy. Thus, the concepts of "amplification" and of "release" enter into the theoretical picture, corresponding with the emphasis laid, in modern biological theories, on the "amplifier" character of the organism (e. g. P. Jordan, 9: "*Verstärkertheorie*"; Mittasch, 10: "*Auslösungskausalität*"). Finally, the

functioning of the effectors is monitored back to the central control system, and this guarantees stabilization or direction of action.

A few important points with respect to feedback systems may be mentioned. First, in order to grant high exactness and swiftness of operation, great amplification should be used in the feedback system. This, however, makes the system unstable with respect to disturbances. Therefore, a compromise between these two necessary and antagonistic needs, sensitivity and stability, must be provided.

Secondly, when in the foregoing we have spoken of "feedback," we have considered what is to be termed more exactly "negative feedback," meaning that the feedback acts in such a way as to damp fluctuations occurring within the system, steadying thus its action and hence being regulative and teleological. But also the contrary is possible, namely, that the information which is fed back is not opposed to, but increases the process in action. This positive feedback will lead to an ever increasing disturbance, and finally to the destruction of the system. Positive feedback corresponds to the vicious circles, well-known in many physiological and psychological phenomena under pathological conditions.

Related to this is the occurrence of ever increasing oscillations which progressively lead a purposeful action astray. An example is purpose tremor in the case of cerebellar lesions. "Offer the patient a cigarette, and he will swing his hand past it in trying to pick it up. This will be followed by an equally futile swing in the other direction, and this by still a third swing back, until his motion becomes nothing but a futile and violent oscillation" (16, p. 113). This is caused by the fact that the feedback is not damped in the cerebellar patients.

Such is the general feedback scheme offered as an explanation of teleological behavior. Cybernetics contends the "essential unity of the set of problems centering about communication, control, and statistical mechanism whether in the machine or in living tissue." We shall not enter here in a discussion of the highly original and fascinating Theory of Communication as developed by Wiener. Our considerations will be limited to only one question, namely, in how far the model proposed may serve as an explanation of biological teleology (for the neurophysiological and psychological aspects of Cybernetics, *cf.* McCulloch and Pitts, 11; Northrop, 12; Hoagland, 5).

* * *

There are, in fact, many biological phenomena which correspond to the feedback scheme. Roughly they may be divided into three classes.

The first is the sphere of those biological phenomena which have been termed *homeostasis* by Cannon. We may define homeostasis as the field of organic regulations which act so as to maintain the steady state of the organism in outside changes and which are effectuated by regulating mechanisms in such a way that they do not occur necessarily in the same, and often in the opposite direction, to what a corresponding change would cause according to general physical principles. The simplest example is homeothermy. According to van't Hoff's rule, decrease in temperature leads to a slowing down of chemical reactions, as it does in ordinary physico-chemical systems and also in poicilothermic animals. In homeothermic animals, however, it leads to the opposite effect, namely, increase of metabolic processes, with the result that the temperature of the body is maintained constant at a level of approximately 37°. This is effectuated by a feedback mechanism: cooling stimulates the thermogenic centers in the striatum and hypothalamus which "turn on" heat-producing mechanisms in the body. Basically similar mechanisms are of course active in many physiological phenomena: maintenance of a constant oxygen, carbon dioxide, and pH level in the blood, maintenance of a constant water content and osmoregulation, i. e., maintenance of a constant total concentration, as well as of the specific concentrations of ions in the body fluid; maintenance of a constant blood sugar level; calcium level regulation by the parathyroid hormone, and so forth. In fact, the rise of homeostatic mechanisms and the progressive independence of changes in environmental factors is a general characteristic of evolutionary progress, as stated already in Claude Bernard's classical dictum: "*La fixité du milieu interieur est la condition de la vie libre.*" Even technological progress in mankind can be considered a continuation of the same trend: by means of dwellings, of agriculture, of social organization, etc., man creates a "cultural environment," making him independent of the fluctuations of the seasons, climates, natural resources, and so forth.

Comparable principles of feedback apply to the regulation of posture: in the way of proprioceptive excitations, information of occurring disturbances is fed back to the centers concerned, and so posture is maintained.

A second group which may be compared to servomechanisms in technology, as, for instance, self-directing missiles, is the *regulation of actions*. If we want to pick up a pencil, report is made to the central nervous system of the amount of which we have failed the pencil in the

first instant. This report is partly visual, if we follow the motion with our eyes; mainly it is proprioceptive. This information is monitored back to the central nervous system so that the motion is controlled till it reaches its aim. If kinesthetic control is lacking, the patient shows *ataxia* as in *tabes dorsalis* where the kinesthetic sense is damaged, and no more messages from the receptors in joints, tendons, muscles, soles, etc., come in. Thus, many activities of the central nervous system do not follow the simple stimulus-response scheme, but are circular processes. The principle is of a wide application. In animal behavior, the goal, say a prey the animal is hunting, can be reached in different ways, and is approached in continuous adaptation to individual and changing situations, and even by different and changing modes of locomotion, such as running, swimming, flying, etc. Thus, even simple locomotions do not follow the stereotype stimulus-response pattern as upheld by tropism theory: only the goal to be reached is a fixed point, the reactions leading to it are variable and plastic. This, however, becomes understandable by the continuous control of actions through signals fed back into reverberating circuits, and has a mechanical model in self-guided missiles.

The third group is "drives." Recent research has shown that automatic activity, rather than reflex action, is to be considered the primal element in behavior (von Holst, Lorenz, Coghill, and others; *cf.* the discussion in 2, p. 111 ff.). According to Lorenz, instinctive actions are essentially based upon pre-established sequences of impulses ("genetic coordinations") which can be discharged even without external stimulus ("running-idle reactions"). It is an essential improvement of this picture to understand that the action, once started by internal conditions, is directed to its goal in the way of feedback control.

* * *

There is, however, another scheme which can be applied as a model for biological regulation. It is the conception of the organism (and other self-regulating units) as systems of dynamically interacting elements. Basic to this conception are kinetic and dynamic principles of equilibria or pseudo-equilibria to which the system as a whole is tending. With respect to the living organism in particular one of its most profound characteristics is found in the fact that it is an open system, i. e., a system exchanging materials with its environment, a singular condition being given as a steady state which the system (certain conditions presupposed) is attaining.

In contrast to the consideration of organic regulations as based upon structural arrangements, the system conception is based upon dynamic interactions. The consideration of organisms as open systems leads to the result that the form of teleology which is most characteristic of organic systems, and has been considered the most important proof of vitalism, namely, equifinality, is, in fact, a characteristic of open systems in so far as they attain a steady state. Equally, other important vital characteristics, such as self-regulation, the so-called anamorphosis (i. e., the transition toward higher states of order, as opposed to the general tendency towards maximum disorder, as demanded by the second law of thermodynamics) and others, are, in fact, consequences of open-system principles. On the other hand, this conception has led to quantitative laws in the fields of metabolism, growth, morphogenesis, and excitation.

Both conceptions, Cybernetics and theory of open systems, aim at the same goal, namely, scientific explanation and laws of organic "wholeness" and teleology. It appears that, at least at present, there are no other basic theories in this field. Thus, it might be well to state the differences, the merits, and the limitations of both theories.

* * *

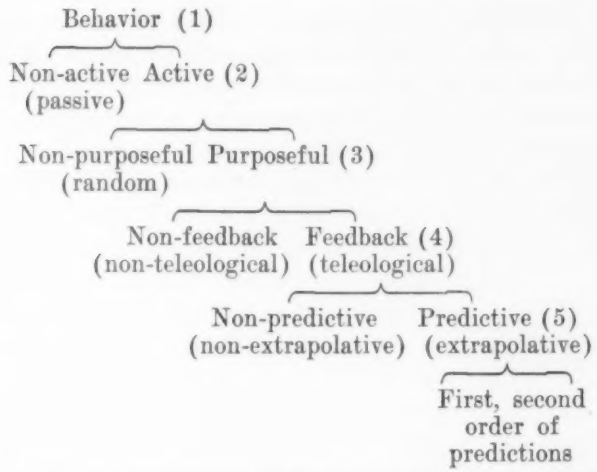
Immediately we see the similarities as well as the differences between both conceptions. In both cases, the system develops forces directed against the disturbance coming from outside and tending to re-establish a normal state. But the causal basis is different. According to the system conception, "teleological" behavior results from dynamic interaction within a unitary system that attends certain conditions of equilibrium or steady state. The compensation of disturbances coming from outside is the result of kinetic and dynamic counteraction of the type of Le Chatelier's principle, which can be expanded and generalized also to open systems. According to the feedback scheme, "teleological" behavior, i. e., a behavior either maintaining the system itself (homeostasis) or attaining a goal outside the system (a target reached by steering mechanisms) is due to pre-established structural arrangements.

The model scheme in feedback is essentially a machine theory. This is emphasized also by its authors. Applying notions from the field of communication to the study of behavior of living organisms "is to treat the nervous system like an automaton." In an amusing passage, Wiener surveys the evolution of automata both as mechanical constructions and as intellectual schemes for the explanation of life. The automata of the

17th and 18th centuries, as well as the model-conceptions of life, have been clockworks. The dancing figures of the great clocks of the cathedrals or on the top of music boxes, and also the orreries imitating the solar system, performing their predestined dance, correspond to Leibniz's monads. The 19th century automaton is a glorified heat machine, burning some combustible fuel instead of the glycogen of the muscle. The present study of automata, whether in the metal or in the flesh, is a branch of communication engineering, introducing the central notion of " message," and thus takes into account the basic characteristic of living organisms, namely, their awareness of the outer world. Their respresentatives are mechanical brains, solving differential equations, roboters, pointing guns to the place at which a radar beam picks up an airplane, and the like. According to McCulloch, the Conference on Teleological Mechanisms " has considered two related questions; namely: ' What characteristics of a machine account for its having a *Telos* or end, or goal?' and ' What characteristics of a machine define the end, or goal, or *Telos*?' It has answered the former in terms of activity in closed circuits, and the latter in terms of entities which the mechanism could compute in terms of discriminations it could make " (Conf., p. 259).

Here the two basic features of the theory are obvious, namely, first the machine model of biological phenomena, secondly, the restriction to closed systems. In this sense, our theory seems to be more revolutionary, since, first, it overthrows, as far as basic phenomena are concerned, the Cartesian machine-theory of the organism, and, secondly, is based upon the conception of the organism as an open system.

This entails another consequence. Feedback mechanisms are essentially one-way causal chains. This is not altered even in the case of what may be called compound feedback, i. e. when not only one, but some regulating mechanisms are active, as is the case, for instance, in ship-steering systems. Feedbacks are what has been called by H. Jordan (8) " amboceptors." In many biological regulations, however, we have pantoceptor reactions, i. e., interactions of many or all elements integrated in the system. That dynamical order is not taken into consideration by Cybernetics, is made clear in an earlier publication (Rosenblueth, Wiener and Bigelow, 14). Here the following scheme is given:



(1) Behavior = " examination of the output of an object and of the relations of this output to the input."

(2) Active = behavior " in which the object is the source of the output energy involved in a given specific reaction."

(3) Purposeful = " the behavior may be interpreted as directed to the attainment of a goal." Not all machines are purposeful, e. g., a gun is not " purposeful " for it can shoot at random; but a torpedo with a target-seeking device is purposeful.

(4) Teleological behavior means purpose controlled by negative feedback. Non-teleological behavior results from positive or non-damped feedback (e. g., *circulus vitiosus*, purpose tremor).

(5) Predictive behavior is, e. g. that of a cat which foresees the future movements of a mouse. Non-predictive is the behavior of an ameba controlled only by the actual stimulus constellation.

* * *

Let us see which phenomena seem to submit themselves to the two model conceptions. We have already said that a wide field of organic regulations can be subsumed under the general feedback scheme, this field comprising especially homeostasis, control of action, and drives based upon autonomous activity.

But the primary organic regulations do not fit into any machine-conception in both its aspects, namely, structural conditions for the order of processes, and one-way causality. In fact, these very phenomena have been considered " proofs of vitalism." This applies to the classical " proofs of vitalism " as advanced by Driesch and others, such as equifinal

regulation in embryonic development, self-regulation in metabolism, "remaining a whole" in spite of division in biological elementary units such as genes and chromosomes, as well as to regeneration, wound-healing, regulations within the nervous system after injuries in its peripheral and central parts, re-establishment of *gestalt*-perception, etc. Feedback gives no more an explanation of these phenomena than any other machine-theory, whilst these features are, at least in their general aspects, consequences of the open-system character of the organism.

It seems important clearly to distinguish these two principles of explanation since muddle in these concepts easily leads to theoretical misinterpretations and inappropriate experimental attitudes. For instance, Brody (4) gives an excellent survey of regulatory phenomena in the organism. He uses the notion of homeostasis to "describe the tendency to maintain a steady state." "While some phases of adaptation of organisms or superorganisms may be considered as special cases of the theorem of Le Chatelier, formulated for non-living systems, homeostasis is reserved for biological (and sociological in this book) changes which do not necessarily occur in the same direction, as they would in a non-living system." This definition is wrong in both ways. It is too narrow since homeostatic regulation is not limited to biological phenomena, but is a subclass of feedback mechanisms which could be constructed very well in non-living devices for almost any of the parameters maintained constant in the living organism. On the other hand, Brody's definition is too wide. He gives an extensive survey of "homeostatic" phenomena which, in fact, are of a diverse nature. A first group comprises phenomena such as thermoregulation, regulation of thyroxine production, regulation of hemoglobin concentration in the blood at reduced oxygen pressures, maintenance of a constant oxygen and carbon dioxide level in spite of increased oxygen consumption and CO_2 production during muscular work, etc. On the other hand, Brody speaks of "homeostasis" of metabolism in relation to surface area (surface rule of metabolism and the like), or of "homeostasis" of growth: growth proceeds as if the "normal" condition were that represented by the mature size, and the rate of growth tends to be proportional to the distance from the mature size. Pathology, especially wound healing, "may be considered as the study of attempts to maintain homeostasis under conditions of injury." Thirdly, Brody mentions phenomena such as weight regulation, body-water regulation, carbohydrate-level regulation, calcium-level regulation, etc.

Though we are in full agreement with virtually all factual statements of Brody, it seems misleading to unite these phenomena under the head of "homeostasis." The phenomena we have enumerated first appear to be homeostasis in proper sense, i. e. regulations by means of feedback mechanisms. The second class, such as regulation of metabolism, growth, and the like, is not homeostasis, but based upon open-system principles in accordance with certain physiological and geometrical conditions. The third class comprises phenomena of either mixed or uncertain nature. Take, for instance, the regulation of the oxygen and acid level in the blood. The primary regulation is based upon the chemical equilibrium of hemoglobin and oxygen, the buffer capacities of the blood, etc. Superimposed on this is the mechanism of lung ventilation which is increased when the carbon dioxide concentration is increased in the blood, further the action of the kidney which secretes excessive acids, finally increased production of erythrocytes, increase of hemoglobin and of respiratory enzyme content, etc. Similarly, the blood sugar level is maintained constant because carbohydrate is released from the liver if sugar concentration goes below a certain level. This level is, however, not that of chemical equilibrium, which would signify a practically complete breakdown of sugar. This means probably maintenance of a steady state in an open system, though unfortunately we do not have a kinetic description of this fundamental regulation. In a somewhat similar case, however, namely acetylcholine content of the brain, Hobbiger and Werner (6, 7) have stated that it corresponds to a steady state. On the other hand, in the maintenance of the blood sugar level, insulin is essentially involved, and so is the nervous system, especially the sympathoadrenal system, in the case of emergency. This is homeostasis proper and feedback. In phenomena of this kind, we seem to have dynamic steady state regulations, on which are superimposed mechanisms of a feedback character.

Thus we do not agree with Brody if he refers to "homeostasis as the *major* manifestation of what is sometimes referred to as the "'field' formulated by the organismic theory in biology." This would only be allowed if we use the term "homeostasis" in a loose way in which it loses its proper meaning.

The distinctions made have important methodological consequences. Where we have homeostasis, we must unveil the mechanisms involved. But where we have purely dynamical regulations, the consideration of phenomena as "homeostasis" leads us astray since we look for mechanisms where there are none. This is a positive danger which has muddled biological experimentation and theory in many cases—from Weismann's

machine theory of embryonic development, the inadequancy of which has led to neovitalism, to the shortcomings of the classical theory of nerve centers and brain localization, and to urgent problems in cell physiology.

On the other hand, we find in the report of the Conference of Teleological Mechanisms a paper on "circular causal systems in ecology" by Hutchinson, exposing the regulations in population systems on the basis of modern mathematical theory, as developed by Volterra and others. This has nothing to do with feedback in proper sense. For Volterra's theory is essentially kinetic (and dynamic for that matter, in an aspect not reviewed by Hutchinson). Ecological equilibrium is *not* feedback, as this notion is used with respect to technical devices, such as Watt's governor, thermostats, servomechanisms, etc., or to proprioceptive control, homeostasis, and so forth.

* * *

Similar considerations apply to the *gestalt* problem. We shall not enter into a discussion of the fascinating implications of Cybernetics with respect to the comparision of computing machines and the brain, the relations of arithmetic and logic, and so forth. Rather we are limiting ourselves to the question in how far the scheme in question is sufficient for the explanation of *gestalt* phenomena.

According to Wiener (16, pp. 160 ff.), *gestalten*, e. g., different perspective views of a figure recognized as the "same," form a transformation group in the sense of group theory. Now in ordinary television a region of a two-dimensional plan is covered by the process of scanning, consisting essentially in taking a nearly uniformly distributed set of sample positions in that region as representing the whole. In a similar way, every region in a group-space of any number of dimension can be represented by a process of group scanning, meaning that any of the positions in this space is traversed in a one-dimensional sequence, the net of positions being so distributed that it comes near to every position in the region. These positions or sets of parameters will generate any transformation desired by using an appropriate transformation operator. If the scanning is fine enough, and the region transformed has the maximum dimensionality of the regions transformed by the group desired, the transformations actually traversed will give a resulting region overlapping *any* transform of the original region by an amount which is as large a fraction of its area as we wish. Thus, if we have a fixed comparison region and a region to be compared with it, and if at any stage

of scanning the image of the latter coincides with the fixed pattern in a degree surpassing a given tolerance, this is recorded as the two regions being alike. If this happens at no stage of the scanning process, the regions are recorded as being unlike. This process can serve as a method to identify the shape of a figure independently of its size, its orientation, or other transformations, and is perfectly adapted to mechanization. Such a device for group-scanning, planned as a prosthesis for the blind, and registering the *gestalten* of printed letters by means of photoelectric cells and by an arrangement of tones of different pitch, was developed by McCulloch. The scheme of this arrangement resembles the arrangement of neurons and nerve connections in the fourth layer of the visual cortex.

It appears, therefore, that the recognition of *gestalten* in different size, perspective, etc. can be explained in the way of machine-arrangements, or, as we may put it, of analysis into punctual events and processes in fixed paths. A similar conception of a "thinking machine" has been indicated already by Rashevsky (13). But it seems questionable in how far the scheme offered is also applicable to the well-known dynamic aspects of the *gestalt* problem.

For example, if, for a fraction of a second, nine points in a circular arrangement are presented to the eye, and a tenth somewhat outside of the circle, this outside point seems to wander into the circumference of the circle in order to complete the most significant or *prägnant gestalt* possible. Or if momentarily presented figures show little gaps, movements to close the gaps are seen—the free ends of the figure flash together. If a stick is so arranged that its projection on the retina goes through the blind spot, it is seen without any gap, whereas if a man is so placed that the projection of his head falls on the blind spot of the observer's fixated eye, the head is not seen. The reason is that only *gestalten* representing definite geometrical figures can be completed.

In such examples, which could be continued almost indefinitely from well-known experiments of *gestalt* psychology, it appears that *gestalt* perception is governed by dynamic laws of the *gestalt*-as-a-whole, as emphasized from the beginning by Köhler and other *gestalt* theorists. It is to be asked therefore, whether, and how, the principles of Cybernetics may account for these very basic characteristics.

* * *

It appears, therefore, that we have to distinguish two different classes within the phenomena of regulation, finality, directiveness, and *gestalt*. So far as we see, the *primary* regulations are such as result from dynamic interaction within a unitary system and according to general system laws. Superimposed are those regulations which may be called *secondary*, and which are governed by fixed " machine-like " structures. They comprise all phenomena of feedback, homeostasis, and related regulative mechanisms. There is a simple reason for the primacy, in the organism, of dynamical as against structural order. In contrast to a machine which consists of lasting parts, all organic structures are maintained in a continuous process, in perpetual breaking down and regeneration. The only thing which is really permanent in the organism is the order of processes. Therefore, the ultimate reason of the pattern and order in the living system can be sought only in the laws of the process itself, not in pre-established and enduring structures (on the limitations of structural explanations of vital phenomena, *cf.* 2, p. 26 ff.). Ontogenetically as well as phylogenetically, we find a principle of progressive mechanization, creating those mechanisms and making the organism more efficient while at the same time limiting its regulability (2, p. 27 ff., 53, 113). Dynamic regulation appears to be the primordial and more general case.

This does not mean, of course, that there is a sharp borderline between " dynamical systems " and " machines." The physical laws are the same everywhere. The construction of a " machine " essentially means that, within a system of forces, conditions of constraint are introduced, so that the degree of freedom in certain causal chains is restricted, usually so that only one course is possible, and causality runs one way. But even in man-made machines, we have all transitions from fixed to loose coupling, allowing for only one, or for several degrees of freedom. But this is precisely what we mean and what we find in nature. The primary and basic regulations seem such as in systems with a minimum of constraint and therefore a high degree of freedom. Ontogenetically and phylogenetically, conditions of constraint evolve, making, on the one hand, the system more efficient, but limiting, on the other, dynamic interaction, degree of freedom, and regulation after disturbance.

What we wish to emphasize is that apart from machine-like structures, general dynamic principles imply principles of regulation which today begin to be unveiled, and which seem to be basic in organic regulations. In this sense, feedback represents an important, but special type of system behavior. " Dynamics " is the broader theory since we can come, from general system principles, always to regulations by machines, intro-

ducing conditions of constraint, but not *vice versa*. In this sense, Cybernetics is part of a General System Theory of the future.

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HUMAN BIOLOGY

a record of research

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FOUNDED BY RAYMOND PEARL

Edited by

CHARLES P. WINSOR

in association with

A. W. FREEMAN — PHILIP E. SARTWELL

CURT P. RICHTER — ADOLPH H. SCHULTZ

Managing Editor

FRANCES D. NICOLLS

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